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In This Issue

The patrician regime of Renaissance Venice was renowned for its stability and preference for age and experience in its officialdom. Yet, young patricians were a visible and unstable presence in political life and occasionally created uproars in the Council. STANLEY CHOJNACKI explains that this youthful misbehavior accompanied an effective system for socializing young men in the collective values of the ruling elite. By the fifteenth century, this socialization had become highly ritualized in rites of passage that offered powerful inducements to young men to conform to the prevailing political discipline. The rituals also reaffirmed for adult society the collective values of the patriciate and so contributed to the political stability for which the regime was known.

SUSAN O'BRIEN illuminates the international links among the Protestant revivals that occurred in the mid-eighteenth century in the American colonies, Scotland, England, and Wales. Historians have noted the similar pattern of events in these different locales, but the major interpretive traditions have focused on the revivals in specific national and denominational contexts. O'Brien argues that international contacts were important to the success of the local movements and that the transatlantic community of evangelicals and the methods of communication its members developed marked a significant stage in the history of evangelical Protestantism.

A debate has long raged among historians of demography and public health about the cause of the first phase of the European mortality decline, which lasted from approximately 1740 to 1820. One of the two main schools in the debate holds that improved nutrition permitted greater numbers of people to survive. The other school argues for the effects of newly instituted measures of public health and hygiene. The absence before the 1820s of systematic records of causes of death hinders resolution of the issue and forces scholars to build their arguments on inferences from other types of data. JAMES C. RILEY looks into the role of insects and sees public health measures as a key, although he notes that their greatest effects came indirectly, by reducing the density of insect populations that acted as a point of contact between pathogens and people.

MICHAEL LES BENEDICT surveys the history of common-law copyright and the doctrine of fair use of protected material for scholarly purposes and warns that historians may soon lose access to material crucial to their work. The Copyright Revision Act of 1976 seemed to bring unpublished manuscripts within the doctrine of fair use on the same terms as published material, but, in the decade since the law's passage, the Copyright Office has attempted to narrow the application of fair use in the case of unpublished manuscripts and so to place them outside the photocopying provisions of the act of 1976. A recent decision by the Supreme Court suggests that it is moving in the same direction.

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Political Adulthood in Fifteenth-Century Venice

STANLEY CHOJNACKI

In august 1482, a sixty-one-year-old venetian patrician named Piero Priuli climaxed more than three decades in the government of his city by gaining election as a procurator of St. Mark, an office in Venice's hierarchy of distinctions second only to the highest office, the dogeship itself. Even though Piero was not the most prominent of patricians (or nobles, as they called themselves), he had earned this honor after a career marked by numerous posts of responsibility. Specializing in fiscal matters, he had served a term as one of the three *avogadori di comun*, or state attorneys, and had sat on the Forty, Venice's highest court. His abilities (or connections) were strong enough to get him elected to the Forty when he was only twenty-nine years old—necessitating a six-month wait until he reached the statutory age of thirty before he could take his place on the bench.[1] But Piero Priuli's political distinctions, especially his election as an *avogadore di comun*, have an ironic twist that directs attention to some important but little-noted features of the patrician regime of Renaissance Venice. For his start in government was

The following abbreviations are used throughout the notes:

ASV	Archivio di Stato, Venice
AC	Archivio di Stato, Venice, Avogaria di Comun
BMV	Biblioteca Nazionale Marciana, Venice
BO	Archivio di Stato, Venice, Avogaria di Comun, Balla d'Oro
"Cronaca Zancaruol"	"Cronaca copiata da Gasparo Zancaruol," Biblioteca Nazionale Marciana, Venice, Codici Italiani, classe VII, 2570 (= 12462)
Dieci	Archivio di Stato, Venice, Consiglio dei Dieci, Deliberazioni
MC	Archivio di Stato, Venice, Maggior Consiglio, Deliberazioni
PE	Archivio di Stato, Venice, Avogaria di Comun, Prove di Età
Raspe	Archivio di Stato, Venice, Avogaria di Comun, Raspe
Sanudo	Marino Sanudo il Giovane, *De origine, situ et magistratibus urbis venetae, ovvero La Città di Venetia* (1493–1530), ed. Angela Caracciolo Aricò (Milan, 1980)
Senato, Misti	Archivio di Stato, Venice, Senato, Deliberazioni Miste
Voci	*Archivio di Stato, Venice, Segretario alle Voci*

All citations of manuscript sources in the ASV are to registers. Note: The Venetian calendar year began on March 1st; in this essay, dates have been converted to modern style. Thus, a reference in a source to January 11, 1420 is represented here as January 11, 1421.

[1] Piero was born on January 11, 1421; BO 163, f. 214v. For his elections as *avogadore* (1476) and as procurator, see Voci 6, ff. 7r, 87v. He was elected to the Forty in June 1450 but did not prove his eligibility by age until the following February 1st. Voci 4, f. 126v; PE 178, f. 74v. Among his other offices were *signore di notte* (a police post), *provveditore al sale* (supervisor of the salt trade), *governador delle entrade*, and official of the Rason Vecchie (both finance offices). Voci 4, f. 3v; Voci 6, ff. 46v, 73r, 140v.

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decidedly inauspicious. On August 26, 1442, when Piero was only twenty-one, he and twenty-six other young nobles shocked the Great Council, seat of government and preserve of the hereditary patriciate, by bursting out of the chamber and rushing down the stairs outside, sweeping along with them an *avogadore di comun* who had been guarding the Council door. The "insolence and temerity" of these riotous young men, all but one under the age of thirty and three-quarters twenty-six or younger, clearly merited punishment. At the insistence of the offended *avogadore*, normal criminal procedures were suspended and the young "delinquents" tried and sentenced on the spot. For the "ignominy" their behavior had brought to the government and particularly to the Avogaria di Comun, Piero and his exuberant young companions were banished from Council meetings for six months and given fines of fifty lire each.[2]

The coupling of Piero Priuli's involvement in the fracas of 1442 with his later political career, distinguished as it was by the Avogaria di Comun and the Procuracy of St. Mark, raises questions about the vaunted patrician political discipline that contemporaries and many writers since have seen as the key to the remarkable governmental stability that set Venice apart from the more turbulent states of Renaissance Italy and that forms the substance of the famous "myth of Venice."[3] This apparent anomaly calls for an explanation of the practical efforts by the regime to make such discipline last, beginning with the conditions under which young nobles were integrated into the governmental activity of their class and educated in its political principles. An inquiry into the politics of Renaissance Venice, specifically, an investigation of the process by which young patricians reached political adulthood in the fifteenth century, can help to elucidate certain connections that remain unexplored in our picture of Venetian politics: the connections between Venice's reputation for orderly government and the youthful unruliness in the Council chamber; between the marked favor the regime gave to age and experience and the persistent and sometimes prominent presence of young men on the political scene; and, fundamentally, between the turbulent antics of the young and legislation in the fifteenth century that spelled out the political ideology of the regime. Investigation of these issues reveals the influential presence in Venetian politics of that ethnographic staple, the rite of passage. It also reveals in Venice, as other research has discovered elsewhere in late medieval and early modern Europe, the importance of the symbolic and monitory role of the young in reinforcing the values of a society. And, in a special way, it casts light on

[2] For a full description of the disturbance in the trial record, see Raspe 3649/9, pt. 1, f. 7rv. The ages of the guilty nobles were traced in BO 162 and 163.

[3] For recent discussions of Venice's governmental reputation, see Edward Muir, *Civic Ritual in Renaissance Venice* (Princeton, N.J., 1981), chap. 1; and Robert Finlay, *Politics in Renaissance Venice* (New Brunswick, N.J., 1980), 27–37. For classic statements, see William J. Bouwsma, "Venice and the Political Education of Europe," in J. R. Hale, ed., *Renaissance Venice* (London, 1973), 445–66; and Franco Gaeta, "Alcune considerazioni sul mito di Venezia," *Bibliothèque d'Humanisme et Renaissance*, 23 (1961): 58–75. On contemporary Florentine views of Venice, see the many references in Gene Brucker, *The Civic World of Early Renaissance Florence* (Princeton, 1977); Felix Gilbert, "The Venetian Constitution in Florentine Political Thought," in Nicolai Rubinstein, ed., *Florentine Studies: Politics and Society in Renaissance Florence* (London, 1968), 463–500; and Renzo Pecchioli, "Il 'mito' di Venezia e la crisi fiorentina intorno al 1500," *Studi storici*, 3 (1964): 451–92.

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the significance of the liminal, or threshold, period of young adulthood in the articulation of the patrician political ideology.[4]

The melee of 1442 was far from the only episode of youthful shenanigans in the Great Council. Four years later, another group of patricians—eleven this time, all but two of them younger than twenty-five—threw the Council into an uproar with another hasty departure. In 1460, forty-one "nobiles juvenes" burst out of the chamber, once again buffeting, with "maximum impetum," the *avogadore* guarding the door. These clamorous events, in their "violent fury" (as the indictment of 1460 put it), offended the government's dignity.[5] But though the most spectacular, they were not the only youthful floutings of orderly procedure. In 1443, the year after the unseemly flight of Piero Priuli and company, "a multitude of noble boys" who had no business being in the chamber disrupted elections by chatting with voting members. Invasion of the Council by underage nobles was a perennial problem; in 1449, they even presumed to cast electoral ballots. The chronic misbehavior of youngsters was so serious that in 1474 the Council doubled the penalties levied on "noble youths" who brazenly flouted their suspensions for previous misdeeds and "disobediently and presumptuously" attended sessions.[6]

These episodes leave no doubt about the presence of youths in the fifteenth-century Great Council, the records of which speak plainly of the "noble youths" or even "noble boys" whose antics disrupted sessions.[7] Closer examination underscores how young they were. It is possible to determine the ages of sixty-one of the seventy-nine nobles who fled the Council chamber in the eruptions of 1442, 1446, and 1460. Unlike the bothersome underage visitors, these nobles were all qualified members of the Council. Yet, fifty were twenty-eight years of age or younger and fully half were still in their early twenties.[8] In a political class famous

[4] For important recent analyses of the issues in Venetian Renaissance politics, see Finlay, *Politics in Renaissance Venice*; Gaetano Cozzi, "Authority and the Law in Renaissance Venice," in Hale, *Renaissance Venice*, 293–345; Felix Gilbert, "Venice in the Crisis of the League of Cambrai," in Hale, *Renaissance Venice*, 274–92; and Muir, *Civic Ritual in Renaissance Venice*. On the preference for mature government officials, see Robert Finlay, "The Venetian Republic as a Gerontocracy: Age and Politics in the Renaissance," *Journal of Medieval and Renaissance Studies*, 8 (1978): 157–78. On the role of the young elsewhere in Europe, see Natalie Zemon Davis, "The Reasons of Misrule: Youth Groups and Charivaris in Sixteenth-Century France," *Past and Present*, no. 50 (1971): 51–91; Jacques Rossiaud, "Prostitution, jeunesse et société dans les villes du Sud-Est au XV^{e} siècle," *Annales: Économies, Sociétés, Civilisations*, 31 (1976): 289–325; Georges Duby, "Au XII^{e} siècle: Les Jeunes dans la société aristocratique," *Annales: Économies, Sociétés, Civilisations*, 19 (1964): 835–46; Richard C. Trexler, *Public Life in Renaissance Florence* (New York, 1981) and "Ritual in Florence: Adolescence and Salvation in the Renaissance," in Charles Trinkaus and Heiko A. Oberman, eds., *The Pursuit of Holiness in Late Medieval and Renaissance Religion* (Leiden, 1974), 200–64; and Christiane Klapisch-Zuber, "The 'Mattinata' in Medieval Italy," in *Women, Family, and Ritual in Renaissance Italy*, trans. Lydia G. Cochrane (Chicago, 1985), 261–82.

[5] The indictment asserts that the regime, and, specifically, the Avogaria di Comun, suffered "great ignominy" and the whole Great Council "no small scandal" as a consequence of the young miscreants' "presumption, disobedience, assault, and violent fury [impetu ac violento furore]." Raspe 3651/11, ff. 80v–81r (March 16, 1460). For similar language in the 1446 episode, see Raspe 3649/9, pt. 2, f. 14r.

[6] MC 22, Ursa, ff. 148v (1443), 173v (1449); MC 23, Regina, f. 138rv (1474).

[7] "Multitudo puerorum nobilium," MC 22, Ursa, f. 148v; "nobiles juvenes," MC 23, Regina, f. 138rv. *Nobiles juvenes* is also used in the Raspe trial records as a blanket term for the boisterous Council fugitives.

[8] Twenty-four of the young men were younger than twenty-five years; twenty-six were between the ages of twenty-five and twenty-eight; seven ranged from twenty-nine to thirty-two; three more were thirty-three or thirty-four; and the remaining one was thirty-eight years old. The ages are in BO 163 and 164.

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for favoring age and experience in its officials, a large cohort of young men nevertheless took part in the affairs of government. For example, when the Great Council met to elect a new doge, an exercise from which members under thirty were excluded, voting totals dropped by about 12 percent, an indication that one of every eight of the active Council members was in his twenties.[9]

The political participation of young men was not limited to meetings of the Great Council. Though suspended from government business for six months, Piero Priuli and his disorderly mates returned to plunge into active involvement in affairs of state. Two years after joining in the uproar of 1442, the young patrician Paolo Zane was elected to the high Court of the Forty; the same honor went to Luca Pisani two years after he fled the chamber in the disturbance of 1446. Piero Balbi, one of the fugitives of 1460, was given in 1462 the frontline assignment of captain of the Venetian garrison at Brescia.[10] These three were not exceptional. In a group of fifteen young men involved in the three episodes (members of a sample of 1,065 men from sixteen patrician houses who entered adulthood in the fifteenth century and whose political careers I am analyzing in detail[11]), twelve went on to hold elective office, eight gaining election to the Court of the Forty, among other important offices.[12] Six had already held minor posts before their flight, in most cases, civil court advocacies earmarked for men in their early twenties.[13]

Participation of youths in government is a seldom noted but intriguing feature of the Venetian patrician regime. Why, given young men's "scant sense of the Great Council's dignity," as the indictment of 1442 read, were men under thirty not simply excluded from the Council altogether and disqualified from the responsible posts that many later filled? The idea had precedent and support. The Council barred members in their twenties from ducal elections, and at least one commentator, the bilious Domenico Morosini, proposed in the 1490s shutting men under forty out of governmental deliberations.[14] Yet, far from closing the doors

[9] At the ducal election of 1462, the number of voters dropped from 1,063 to 930 (12.5 percent) when the councillors younger than thirty left the hall; at that of 1485, from 679 to 601 (11.5 percent). MC 23, Regina, f. 44v; MC 24, Stella, ff. 60r–61v.

[10] On Paolo Zane, see PE 178, f. 8r; on Luca Pisani, see Voci 4, f. 123v. Piero Balbi's tenure as Captain at Brescia had been preceded by his election the previous year to the financial office of the Misseteria; PE 169, ff. 90r, 106v. All three held other offices in later years.

[11] The houses, or clans, in the sample were chosen to represent numerically small, medium, and large representatives of the categories into which contemporaries grouped patrician houses according to their antiquity: the *case vecchie*, or old houses, the *case nuove*, or new houses, and an influential subgroup of the latter, the *case ducali*, or ducal houses, which monopolized the dogeship in the fifteenth and sixteenth centuries. For the comments of contemporaries on these distinctions, see Finlay, *Politics in Renaissance Venice*, 92–96; Chojnacki, "In Search of the Venetian Patriciate," 49–50. For lists of the *case vecchie* and *case ducali*, see Samuele Romanin, *La storia documentata di Venezia* (1853–61; 3d edn., Venice, 1972–75), 4: 305–06. The houses in the sample are: Morosini, Polani, Zane (*case vecchie*); Lando, Loredan, Priuli (*case ducali*); Arimondo, Balbi, Da Canal, Muazzo, Da Mula, Navagero, Pisani, Ruzzini, Vitturi, Zulian (*case nuove*). The adult careers of the men of these houses form the basis of a study in progress on young manhood in patrician politics and society, provisionally titled "Public Manhood in Renaissance Venice."

[12] Data from Voci 4, Voci 6, PE 169, and PE 178.

[13] The civil court advocacies were more important as stepping stones into the Great Council than as legal-service offices; Sanudo, 144–45.

[14] Gaetano Cozzi, "Domenico Morosini e il 'De bene instituta re publica,'" *Studi veneziani*, 12 (1970): 405–58, esp. 422–23.

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to the young, the patrician regime continued to welcome even twenty-year-olds into the Council and thus into political life, and gave its youthful members, even ones recently guilty of bringing "ignominy to the regime," important jobs in governmental service. To understand the forgiving nature of the regime and assess the place of young men in patrician politics, one must keep in mind that the patriciate was a multiform structure. It possessed several parts: a public governmental dimension (the patriciate as a political class); a scattered, multifamilial, multilineage social dimension; and a cultural dimension of values and symbols that gave it unity and coherence. All three of these were molded by a fourth dimension, the chronological, in which the patriciate reached a major turning point toward the end of the fourteenth and beginning of the fifteenth centuries.[15] The integration of young nobles into political adulthood illuminates each of these dimensions and cannot be fully understood without reference to them all.

First, the perspective of government can be understood partly as a result of its huge growth in this period. From the late fourteenth century, the state was involving itself in an ever-widening range of political, economic, and, especially, social concerns. Among examples of social concerns were policies to regulate marriage, dowries, and display; to tighten control over corporate bodies such as the *scuole*, or religious confraternities; and to oversee the activities of the *compagnie della calza*, young patrician social groups that were just beginning to form in the fifteenth century.[16] Since fewer and fewer areas of social activity escaped state intervention, it is not surprising that the government wanted young nobles in the Council chamber, where authorities could keep an eye on them and, when necessary, discipline them for their excesses. Youthful misbehavior in the Council was tiresome, even outrageous, but it was preferable to having young men turn their energies to still more disruptive or even sinister purposes away from increasingly sharp-eyed official scrutiny.

Young patricians were swept up in another aspect of state growth: a massive increase in governmental activity by nobles. By the mid-fifteenth century, the number of offices in the state administration had swollen to more than twice that

[15] The view, stated here, that the patrician regime continued to evolve during the fourteenth and fifteenth centuries runs counter to a persistent tradition that holds it as essentially fixed in composition and ideology by the early fourteenth. For a recent example of the traditional view, see Guido Ruggiero, "Modernization and the Mythic State in Early Renaissance Venice: The Serrata Revisited," *Viator*, 10 (1979): 245–56. For views of continuing development, see Giorgio Cracco, *Società e stato nel medioevo veneziano* (Florence, 1967); Stanley Chojnacki, "In Search of the Venetian Patriciate: Families and Factions in the Fourteenth Century," in Hale, *Renaissance Venice*, 47–90; and John Easton Law, "Age Qualification and the Venetian Constitution: The Case of the Capello Family," *Papers of the British School at Rome*, 39 (1971): 125–37.

[16] On governmental regulation of marriage and dowries, see Giulio Bistort, *Il Magistrato alle Pompe della Repubblica di Venezia* (1912; reprint edn., Bologna, 1969), 90–113. On patrician marriage, see Bianca Betto, "Linee di politica matrimoniale nella nobiltà veneziana fino al XV secolo: Alcune note genealogiche e l'esempio della famiglia Mocenigo," *Archivio storico italiano*, 139 (1981): 3–64; and Stanley Chojnacki, "Dowries and Kinsmen in Early Renaissance Venice," *Journal of Interdisciplinary History*, 5 (1975): 571–600. On the *scuole*, see Brian Pullan, *Rich and Poor in Renaissance Venice* (Cambridge, Mass., 1971), pt. 1. On the *compagnie* (which merit a modern study) see Lionello Venturi, "Le Compagnie della Calza (sec. XV–XVI)," *Nuovo archivio veneto*, n.s., 16 (1908): 161–221, 17 (1909): 140–233.

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of a century earlier: 286 posts in the 1450s against 141 in the 1350s.[17] According to the diarist Marino Sanudo, writing in the 1490s, out of a population of 2,600 male nobles, close to one-third, some 800, were serving in government posts at any one time.[18] The increased opportunities for government service pleased many individual patricians whose interests were served. But, inevitably, some of the new jobs held little appeal. The result was a recurrent problem of men avoiding onerous or unrewarding posts, however necessary the offices may have been as cogs in the expanding bureaucratic machinery. Among the government's solutions were the carrot of bigger stipends and the stick of sanctions for electees unwilling to serve.[19] A third and more appealing solution was to name younger members of the Great Council to these low-level jobs, an arrangement that also served practical and ideological ends. These young men would hold apprenticeships in government and take their socializing first steps up the political *cursus honorum*—or, as we shall see, *lucrorum*.[20]

In practice, young nobles were not always willing to serve their time patiently at the bottom of the ladder. Most perforce did so, but some hankered after better-paying, more influential, or more prestigious posts. In 1433, for instance, this ambition spawned a clandestine caucus of thirty-four young patricians who organized into a voting bloc to elect themselves to desirable offices. Complaints about young men usurping posts reserved for their elders run like a bright thread through fifteenth-century legislation, as the Council strove perennially to tighten the enforcement of age requirements for different offices.[21] The impatience of young patricians to get beyond tiresome, poorly paid posts and their frustration at being confined to them may have triggered the eruptions out of the Council chamber by Piero Priuli and other youths. Although patricians in their twenties may not have liked the role reserved to them in the government, this service did have one important effect: it taught them to direct their ambitions toward the formal political arena that gave the patrician regime its structure.

[17] These figures are drawn from Voci 4 and 6. They do not include ambassadorships, temporary military posts, or *ad hoc* commissions, nor do they take account of multiple tenants of individual offices such as the court of the Forty, whose membership was expanded in 1400 to eighty, in 1442 to 120, and in 1492 to 160, as the court ramified into specialized jurisdictions. See Heinrich Kretschmayr, *Geschichte von Venedig*, 2 (Gotha, 1920): 103.

[18] On the patrician population, see Sanudo, 146; on the number of office-holders, see Finlay, *Politics in Renaissance Venice*, 59. The office-holder number (831, to be exact) refers to the number of men in office, not to the number of offices.

[19] On penalties for avoiding office, see Donald C. Queller, "The Civic Irresponsibility of the Venetian Nobility," in David Herlihy, *et al.*, eds., *Economy, Government, and Society in Medieval Italy: Essays in Honor of Robert L. Reynolds* (Kent, Ohio, 1969), 225–35. The need to attract officials was constantly cited in preambles to acts raising stipends; see, for example, MC 20, Novella (copy), f. 5v (April 18, 1350); MC 21, Leona, ff. 61v–63v (August 10, 1392); MC 22, Ursa, f. 12v (September 3, 1415); MC 24, Stella, f. 59rv (September 11, 1485).

[20] In an indication that office-holding by young men was viewed as serving more to provide political conditioning for them than to contribute importantly to the functioning of the state, the Great Council in 1487 lowered the age requirement of *capi sestieri* from thirty to twenty-five, "in view of the insignificance [pocha importantia] of the office." MC 24, Stella, f. 91v.

[21] For examples of such complaints, see MC 22, Ursa f. 152r (1443); Dieci, Miste 15, ff. 46v–47r (1455). The plot of 1433 will be treated in a separate study; it is described in "Cronaca Zancaruol," ff. 142v–143r. The ages of the conspirators were drawn from BO 162 and 163.

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The tug-of-war between older and younger rivals for choice offices shows that, for adult patricians of all ages, governmental expansion was less a burden to be borne than an opportunity to be ardently and competitively grasped. In this aspect of government, the patrician monopoly of political activity intersected with the private interest of family and lineage. If the state had its reasons for patiently and tolerantly easing young men into political life, the families also had theirs. From the late fourteenth century, the families came increasingly to depend on the tangible benefits of membership in the ruling class. Economic strains from the mid-fourteenth century onward had caused hardship for nobles, as for other Venetians. But hardship became desperation under the economic impact of the War of Chioggia against Genoa and its allies (1378–81). To sustain the war effort and fight off the Genoese besiegers at the very edge of the Venetian lagoon, the government remorselessly raided the holdings of nobles and citizens alike with forced loans throughout the war years. These exactions had a devastating effect, producing, in Gino Luzzatto's phrase, an upheaval in private—including patrician—wealth.[22] Even worse, long-term pressures choking off traditional economic activities, especially in the Levantine trade, deprived beleaguered nobles of opportunities to recover in the marketplace. The economic distress produced two important results: a governmental policy of providing economic assistance to needy patricians and patrician selfishness about sharing this assistance.

In the 1380s, the government instituted a welfare state for hard-pressed nobles. One component directly addressing the fiscal woes pressing on many patrician families was leniency for truly needy men who fell behind in their impost payments. Nobles who proved genuine insolvency could spare themselves the loss, mandated by earlier legislation, of their noble privileges.[23] The government increased the opportunities for nobles to gain appointments as crossbowmen on state-run merchant vessels, posts that offered the poorer young nobles free passage to distant ports of call where they could make a little money by trading.[24] But the principal form of governmental succor, serving patricians of all ages, was political office. Most scholarly discussion of government posts as noble poor relief has focused on the late fifteenth century.[25] But as early as the 1380s, the Great Council wished to spread the benefits of office throughout the class and asserted its

[22] On the economic impact of the War of Chioggia, see Gino Luzzatto, *La storia economica di Venezia dall'XI al XVI secolo* (Venice, 1961), 144–45; Reinhold C. Mueller, "Effetti della Guerra di Chioggia (1378–1381) sulla vita economica e sociale di Venezia," *Ateneo veneto*, n.s., 19 (1981): 27–41; and Roberto Cessi, "La finanza veneziana al tempo della guerra di Chioggia," in *Politica ed economia di Venezia nel Trecento*, 179–248, esp. 195. On economic strains throughout the Trecento, see Luzzatto, *ibid.*, 135–39; Roberto Cessi, "Introduzione storica" to *La regolazione delle entrate e delle spese (sec. XIII–XIV)*, Documenti finanziari della Repubblica di Venezia, ser. 1, vol. 1, pt. 1 (Padua, 1925); and Cracco, *Società e stato nel medioevo veneziano*, chap. 4.

[23] Any noble able to persuade two-thirds of the Forty that he was "fiscally destitute [impotentem ad solvendum . . . imprestita et impositiones]" could keep his seat in the Great Council and, crucially, his eligibility for office. MC 20, Novella (copy), ff. 434v–435v (October 3, 1383).

[24] Senato, Misti 52, ff. 33r (1417), 84v (1418); Senato, Misti 58, f. 81v (1431). On the bowmen, see Finlay, *Politics in Renaissance Venice*, 69; and Law, "Age Qualification and the Venetian Constitution," 131–34.

[25] See Cozzi, "Authority and the Law in Renaissance Venice," 298–300; Finlay, *Politics in Renaissance Venice*, 59–81.

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responsibility to provide for needy nobles by giving them political jobs.[26] It even frankly accepted that some minor posts were the monopoly of "our poorer nobles," who depended on them to support their families; the Council therefore raised the stipends of these offices to make them adequate to family needs.[27] From the late fourteenth century on, patrician reliance on political office was an abiding concern of government, which throughout the fifteenth century responded with legislation that expanded office-holding opportunities for nobles.[28]

The combination of noble dependence on political office and the increased readiness of the government to provide positions marked a new stage for Venice's ruling class. The implications of a large dependent nobility extend to wide areas of Venetian history in the fifteenth century, not least to the expansionist push that made Venice a mainland power.[29] But a more immediate effect appeared in the beginnings of a new exclusivism, which was expressed in significant new measures to fix the patriciate's membership and give new precision to patrician status. In the decades around 1400, a wave of legislation completed the definition of the Venetian ruling elite that had been outlined a century earlier in the so-called *Serrata*, or closing, of the Great Council.[30] The new measures now spelled out in detail what patrician status was, who had it, and how it was to be demonstrated. In addition to meeting the basic patrilineal qualification put in place shortly after 1300, claimants to noble status had to satisfy government investigators about the legitimacy of their birth and even the social standing of their mothers.[31] The Council devised elaborate new procedures for presenting noble credentials and

[26] The principle is stated clearly in legislative preambles. In one example, the Great Council asserted its intention "that all share in the honors and benefits of our polity [de honoribus et beneficiis terre]"; MC 21, Leona, f. 14r (1385). In another, it noted that "the ducal regime [ducale dominium] is accustomed magnanimously to come to the aid of its needy noble citizens, especially those impoverished through no fault of their own [illis qui non suo defectu ad inopiam devenerunt]"; *ibid.*, f. 22v (1388).

[27] *Ibid.*, f. 61v (1392). This act identified "certain lesser offices to which our poorer nobles are regularly [continue] elected," the stipends of which, however, "are inadequate for a livelihood [nullo modo possunt ex illis vivere]."

[28] In 1442, the Great Council created eleven new rectorships in Venice's Italian mainland territories because, as the act's preamble noted, "it enhances the honor of our regime to provide that our nobles may live under our care [sub umbra nostra]." MC 22, Ursa, f. 141r. In 1488, the election of a second treasurer for Ravenna was authorized because, "since the number of patricians [zentilhomeni nostri] has increased and increases daily, it is appropriate and necessary to ensure that the largest number possible [quel piui numero se pono] may serve in the offices and rectorships of the regime." MC 24, Stella, f. 92v.

[29] There are indications, not yet systematically studied, that mainland expansion, the internal politics of Doge Francesco Foscari (1423–58), and the dependence on government jobs of a large segment of the patriciate were all connected. However, early fifteenth-century Venetian political history still awaits its modern student.

[30] The fundamental studies of the Serrata are by Margarete Merores, "Der grosse Rat von Venedig und die sogenannte Serrata vom Jahres 1297," *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 21 (1928): 33–113, and Frederic C. Lane, "The Enlargement of the Great Council of Venice," in J. G. Rowe and W. H. Stockdale, eds., *Florilegium Historiale: Essays Presented to Wallace K. Ferguson* (Toronto, 1971), 236–74. See also Cracco, *Società e stato nel medioevo veneziano*, 331–50; Chojnacki, "In Search of the Venetian Patriciate"; and Ruggiero, "Modernization and the Mythic State in Early Renaissance Venice."

[31] For legislation addressing the social status of mothers of claimants to noble status, see MC 19, Novella, f. 171v (1376), and MC 22, Ursa, ff. 47v–48r (1422). The latter act required, among other provisions, that the *avogadori di comun* "diligently determine who was or is the mother" of anyone wishing to prove his eligibility for Great Council membership. Natal family names of mothers of Balla d'Oro registrants, however, were not recorded until 1431; BO 162.

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increased fines for would-be nobles who failed the tests.[32] Reflecting the new rigor, the *avogadori di comun*, who had the principal responsibility for testing claims to patrician status, began in the 1380s to investigate these claims more assiduously and to deny accreditation more frequently than ever before.[33] The tightening continued throughout the fifteenth century, culminating in the early 1500s in laws requiring registration of noble births and marriages in what were known as the Golden Books, the *Libri d'Oro*, of the patriciate.[34] Exclusivism was a consequence of nobles' increased dependence on the benefits that their status brought them. Eager to limit the roster of potential competitors, they undertook to limit the size of the patriciate.[35] At the same time, they sought to dignify their entitlement to benefits by tightening the pedigree requirements of noble status, as in the legislation disqualifying sons of low-born women. Raising the barriers against interlopers, these measures also contributed to the emergence of a caste mentality. Men whose economic shakiness made them rely on public welfare in the form of public office asserted their special genealogical qualifications for office, thus giving their dependence a cloak of dignity.[36]

Young patricians became the focus of this sharpened sense of status and the instruments of their families' enjoyment of its advantages. On the practical level, having sons in the Great Council added to the voting power of a family, enhancing its position in the electoral exchange of favors that, as Robert Finlay has recently illustrated, occupied ambitious adult patricians.[37] Once in the Council, the young men themselves became candidates for office, potential conduits between their families and the governmental paymaster. Young Council members also strengthened the social position of their families by the appeal their political enfranchisement gave them as prospective sons-in-law.[38] If the government encouraged young nobles to enter political life in order to oversee them, socialize them, and place them in low-level posts, the families had a complementary material interest

[32] For legislation-tightening procedures, see MC 21, Leona, ff. 169r (1407), 181v (1408), and 241v–242r (1414); MC 22, Ursa, ff. 43v, 45r (1421), and 88r (1430).

[33] The first proof of credentials that I have found in the *avogadori*'s Raspe records, which begin in 1324, does not come until 1342; Raspe 3642/2, f. 29r. Such proofs remained rare for several decades thereafter, picking up frequency only in the 1370s, when the *avogadori*'s research became more elaborate. For an example of thorough investigation of a claimant's credentials, see Raspe 3644/4, f. 27rv (1381). The number of rejections on the basis of illegitimacy grew in consequence of the law of 1376 disqualifying bastards from Great Council membership; MC 19, Novella, f. 171v. By the second decade of the fifteenth century, cases of claimants failing to prove their credentials appear with regularity in the *avogadori*'s records. See Raspe 3647/7, ff. 9v–10r (1417), 65v (1419), 105v–106r (1421).

[34] For the laws requiring patricians to register births and marriages, see Dieci, Miste 31, ff. 101rv, 109v–110r (1506); Dieci, Comuni 2, ff. 14v–17v (1526); AC, Contratti di nozze, Reg. 140/1, unfoliated initial sheets reproducing a Senate act of November 4, 1505. On this legislation, see Giuseppe Maranini, *La costituzione di Venezia*, 2 vols. (1927–31; reprint edn., Florence, 1974), 2: 55–67.

[35] The Ducal Council's rejection in 1403 of a proposal to replace each patrician family that died out with one of the "more worthy" citizen families is symptomatic of the tendency toward exclusivism. ASV, Collegio, Notatorio, Reg. 3, f. 111v.

[36] The growing caste consciousness is apparent in the language of legislation. Uncredentialed interlopers were seen as threatening to "blemish [maculare] the reputation, status, and honor" of the regime or to "denigrate and stain the reputation" of the Great Council. MC 21, Leona, f. 241v (1414); MC 22, Ursa, f. 82r (1430).

[37] Finlay, *Politics in Renaissance Venice*, 59–81.

[38] On cooperation between affines, see Stanley Chojnacki, "Kinship Ties and Young Patricians in Fifteenth-Century Venice," *Renaissance Quarterly*, 38 (1985): 240–70.

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in ensuring that the young men qualified for political office. The bread-and-butter motives that united government and family in promoting the political adulthood of young patricians are only part of the story, however. In the fifteenth century, the role of young nobles transcended practical considerations, as they became the principal focal point of the new self-consciousness that marked the increasingly caste-like patriciate. Cogs in the state machinery and instruments of family strategy, patrician youths were also the symbols of a new patrician culture that found its quintessential expression in an elaborate ritual of passage into political adulthood.

LIKE THE CLASSIC RITE OF PASSAGE FIRST OUTLINED BY Arnold van Gennep, the ascent of Venetian nobles into full adulthood was made up of three stages.[39] Starting with separation from adolescence and ending with incorporation into full-fledged political adulthood, young nobles spent an intermediate period corresponding to what van Gennep and his followers have called the "liminal" or "threshold" phase. The peculiar characteristics of the liminal phase help put into perspective the antics of Piero Priuli and other young patricians in the Great Council and make clearer the part played by the young in the sharpening of the patrician ideology. The role of young men in reminding society of its basic values is a theme dealt with from a variety of perspectives in recent writing. For Victor Turner, liminal youths, collectively denied the attributes of full membership in society while undergoing education in its most sacred values, become, in their liminal purity, in their very incapacity to partake of the divisive concerns of their elders, the beacons of those values.[40] In the European context, Natalie Davis observed French youths, detached from the marital and occupational circumstances that led their elders to violate cultural norms, restating those norms in their ceremonial processions and charivaris, reaffirming rule by misrule.[41] Most closely related to the Venetian case, Florentine youths were seen by Richard Trexler as "ritual objects" on whose liminal innocence Florentines reposed their hopes for the "preservation, even salvation, of the natural and civil order."[42] These insights help put Venetian coming-of-age practices into a broader historical and ethnographic perspective. But the elaboration of these practices and the attitudes they reflect

[39] Arnold van Gennep, *Rites de Passage* (Paris, 1909), published in English as *The Rites of Passage*, trans. Monika B. Vizedom and Gabrielle L. Caffee (London, 1960). For a history and analysis of van Gennep's ideas, see Max Gluckman, "Les Rites de Passage," in his collection, *Essays on the Ritual of Social Relations* (Manchester, 1962), 1–52. See also Meyer Fortes, "Ritual and Office in Tribal Society," in *ibid.*, 53–88.

[40] "The neophyte in liminality must be a *tabula rasa*, a blank slate, on which is inscribed the knowledge and wisdom of the group." Thus "[liminality] can be seen as potentially a period of scrutinization of the central values and axioms of the culture in which it occurs." Victor Turner, *The Ritual Process: Structure and Anti-Structure* (Ithaca, N.Y., 1969), 103, 167. See also Fortes, "Ritual and Office in Tribal Society," 83–84.

[41] She notes, however, that when transplanted from village to city, the French abbeys of misrule lost some of their solidarity. Davis, "Reasons of Misrule," 113. See also Rossiaud, "Prostitution, jeunesse et société," 298–300.

[42] Trexler, "Ritual in Florence," 245 and more generally in *Public Life in Renaissance Florence*. For a discussion of Italian youth groups regulating marriage by means of ritual, in a manner similar to that discussed by Davis for France (above, previous note), see Klapisch-Zuber, " 'Mattinata' in Medieval Italy," esp. 282.

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find their greatest significance in the context of the evolving patrician political and social order in the early Renaissance.

The passage to adulthood began with an exercise that formally took a young man out of the ranks of the *adolescentes* and established his legitimate noble birth.[43] This exercise was his registration for the Balla d'Oro, or Barbarella. A lottery for young nobles who had reached age eighteen, the Barbarella enabled winners to take their hereditary places in the Great Council at twenty instead of waiting until they turned twenty-five, the age at which all men with approved credentials could enter the Council.[44] The Barbarella's original purpose, as stated in the instituting act of 1319, was to provide an orderly alternative to elections as the means of selecting Council members; this act, together with another of 1323, established patrilineal heredity as the keystone of the patrician regime.[45] We do not know much about the Balla d'Oro in the fourteenth century, as it was not important enough for presiding officials to keep records about it. A decree of 1356 calling for a fine of 200 lire for registrants under eighteen indicates nevertheless the eagerness at this time of some families to hurry sons into the Council.[46] Only after 1400, however, did individual and collective concerns merge to make the Barbarella the principal point of departure for the passage to patrician adulthood.

The new collective interest in the Balla d'Oro was proclaimed in a Great Council act of 1414 that dressed it up with elaborate registration procedures and prescribed for the first time careful record-keeping of registrations. The rationale for the new rigor was stated in the preamble. Membership in the patriciate was a "most solemn benefit," so solemn, indeed, that unworthy men who "blemished" the patriciate by entering the Great Council under false pretenses endangered the "conservation of the state and the honor of the regime." Therefore, in view of recent episodes of encroachment, the adoption of new remedies to safeguard the purity of the patriciate was imperative.[47] The language and the vote tally (286 in favor, twenty-two against, with thirteen abstentions) show patrician self-consciousness at a new level of exclusivist intensity, with governmental mechanisms set in motion as the means of keeping unworthy persons out.

[43] The term *adolescentes* is less frequently encountered in the Venetian documents than *juvenes* or *giovani*. One use of *adolescentes* that suggests its contemporary meaning is its application to the charter members of the Modesti, one of the *compagnie della calza*, who drew up their statutes in 1487. The ages of eight of the sixteen signatories are traceable in BO 164: of these, seven were seventeen and one sixteen years old when the statutes were drawn up. The statutes are in the Biblioteca Correr, Venice, Codici Cicogna 3278, fasc. 24, ff. 25–29.

[44] On the Balla d'Oro, see Maranini, *La costituzione di Venezia*, 2: 42–43. Maranini errs, however, in asserting that before 1441, registrants, as opposed to Great Council entrants, had to be at least twenty years old. See also the excellent discussion in Law, "Age Qualification and the Venetian Constitution," 128–33.

[45] AC, Reg. 2, Capitolare, ff. 67v–68r. Frederic Lane suggested that heredity may have been established in fact by 1298; "The Enlargement of the Great Council," 255 and corresponding note 83, pp. 272–73. Merores mistakenly put the act instituting the Balla d'Oro in 1321 rather than 1319; "Der grosse Rat von Venedig," 79.

[46] AC, Reg. 2, Capitolare, f. 69v. This act, passed by the Forty on December 9, 1356, does not appear in the Great Council records.

[47] "Cum beneficium nostri consilii maioris sit solenissimum [*sic*], et pro conservatione status et honoris dominationis nostre . . . de ipso non veniant persone qui habeant maculare famam, statum, et honorem tanti dominii; Equidem cum novis casibus nuperime occursis et sequitis, nova sint remedia adhibenda." MC 21, Leona, ff. 241v–242r (March 18, 1414).

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The Balla d'Oro registration procedure became in the fifteenth century the principal checkpoint for protecting patrician status, because fathers, more eager than ever to speed their sons into political life, hastened to sign them up for early Council admission. The sharpened interest of families in the Barbarella is apparent in the registration trends of 1,065 young men who signed up for the lottery between 1408 and 1497.[48] Between 1408 and 1450, 417 registered; but between 1451 and 1497, the number leaped to 648—an increase of 55 percent. The swelling desire to hurry young men into government shows up even more vividly in a comparison of the periods 1411–30 and 1471–90: during the former, 158 young men signed up, during the latter, nearly twice as many—292 (a 185 percent increase).[49] So powerful a hold did the Barbarella gain on the patriciate that, by mid-century, 85 percent of all nobles who entered the Great Council by whatever means had registered for the lottery, a percentage even more striking considering that the Balla d'Oro was closed to men whose fathers, though eligible, had themselves never joined the Council.[50]

The fullest description of the Balla d'Oro is supplied by Marino Sanudo, who wrote about the ceremony in the 1490s.[51] Every December 4th—St. Barbara's Day, hence "Barbarella"—slips of paper bearing the names of all young men who had proved to the *avogadori di comun* their legitimate patrician birth and attainment of age eighteen were placed into a voting urn (*capello*). Into a second urn were put a corresponding number of ballot balls, of which one-fifth were gilded. The two urns were set up in the Sala del Collegio of the Ducal Palace, where the doge himself extracted one by one the slips with the names of the registrants while a young boy (*ballottino*) simultaneously pulled out ballot balls.[52] The young men whose names were called as a golden ball emerged were winners and could enter the Council when they turned twenty. The others would try again on subsequent St. Barbara's Days, until they drew the golden ball, or were elected to one of twenty posts as advocates in the civil courts of the Ducal Palace (which also bestowed Council membership on twenty-year-olds), or until they took their hereditary places at age twenty-five.[53] The Balla d'Oro drawing was a solemn occasion,

[48] The registrants were members of the sixteen sample houses; see above, note 11. The 1,065 registrations are found in BO 162, 163, and 164. Registration records begin (in BO 162) in 1408, apparently as a result of some retroactive recording after the passage of the 1414 act; BO 164 carries registrations to 1497.

[49] The steady rise is apparent in a breakdown of registrations from 1410 to 1490 into four twenty-year periods: from 1411 to 1430, 158 men registered; from 1431 to 1450, 254 registered; from 1451 to 1470, 252; and from 1471 to 1490, 292.

[50] The law of 1414 restricted the Balla d'Oro competition to sons of Great Council members. Men able to document their legitimate birth to fathers who, though noble, had never formally joined the Council could themselves take Council membership at age twenty-five but were ineligible for the Balla d'Oro.

[51] Sanudo, 145–46. Sanudo's description shows that the spare instructions in the instituting act of 1319 had become elaborated by the later 1400s. For the act of 1319, see AC 2, Capitolare, ff. 67v–68r.

[52] The doge's participation was not prescribed in the act of 1319 or mentioned in that of 1414; however, the latter addresses registration rather than the sortition ceremony itself. The ratio of winners to losers, 1:4, was spelled out in a law of 1441. MC 22, Ursa, f. 138r (November 26, 1441).

[53] On the different routes, see Sanudo, 145–46. Roughly half of the 246 men from the sixteen sample houses who joined the Great Council between September 1444 and August 1464 (118, or 48 percent) entered as winners of the Barbarella, one-quarter (sixty-five, or 26.4 percent) by gaining election as

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dignified by the participation of the doge, whose charismatic presence signaled its importance among the civic rituals of Venice, many of which have been analyzed by Edward Muir.[54] The stakes were high for the young participants and their families; on the luck of the draw rode their hopes of quickly expanding the presence of their family in government and adult society. But the deep solemnity of the proceedings—the fixed date, the location in the chamber where the supreme executive body of Venice, the doge and his council, deliberated affairs of state, the presiding presence of the Venetian polity's most august symbol—also had a powerful socializing effect on young men.[55]

The ceremony stimulated the desire of young men to be among the favored one-fifth and surrounded the prize of Great Council membership with a special aura. Testimony from the seventeenth century tells of family festivities celebrating Barbarella victories. In view of the eagerness of fifteenth-century parents to enroll their sons, it is likely that good fortune on December 4th triggered celebrations in that era as well.[56] For those who lost, especially for those whose hopes were dashed year after year, a sense of great disappointment stimulated even greater eagerness to gain the prize.[57] The luster that these ritual acts imparted to membership in the Great Council helps to explain the otherwise puzzling spectacle of underage nobles flocking to Council meetings. The special aura of early entry also explains why, as Sanudo relates, those forced to wait until age twenty-five to join the Council were called the *tristi*.[58] They and their families suffered a diminution of noble dignity by being lumped together with sons of men who, never in the Great Council, had not formally participated in patrician political life. Failure in the Balla d'Oro implied by association a second-class patrician status.[59]

civil court advocates, and only the remaining one-quarter (sixty-three, or 25.6 percent) on reaching age twenty-five: PE 169, 178.

[54] On the ritual importance of the doge, see Muir, *Civic Ritual in Renaissance Venice*, 256–62. On charismatic qualities of political leaders, see Clifford Geertz, "Centers, Kings, and Charisma: Reflections on the Symbolics of Power," in *Local Knowledge: Further Essays in Interpretive Anthropology* (New York, 1983), 121–46, esp. 124.

[55] Commenting on the significance of fixed dates, Barbara Myerhoff noted that rites of transition are especially helpful in solving social problems and perpetuating the social order by socializing and integrating members "when seasonal rituals are contained within or coordinated with rites of passage. . . . Then the individual initiate is presented with a paradigm for his or her future; collective tensions are overcome, and the calendar, by which the society understands its long and short history, is articulated with transitions being marked for the individual." Myerhoff, "Rites of Passage: Process and Paradox," in Victor Turner, ed., *Celebration: Studies in Festivity and Ritual* (Washington, 1982), 108–35, esp. 112–13. I owe this reference to Lisa M. Fine.

[56] On the seventeenth-century celebrations, see Bistort, *Il Magistrato alle Pompe della Repubblica di Venezia*, 275–76.

[57] For an application to rituals of transition of the theory of cognitive dissonance, according to which the value of the prize is enhanced by the suffering undergone to attain it, see Myerhoff, "Rites of Passage: Process and Paradox," 121. This idea is pertinent to the Venetian instance, where four-fifths of every year's Barbarella candidates were denied the prize, leading to such cases as that of Domenico Priuli, who registered just in time for the lottery of December 1452 but did not gain the golden ball until 1457; or that of Paolo Loredan, who registered in 1453 but also did not win until 1457. BO 163, ff. 216r, 290r; PE 169, ff. 14v, 27r.

[58] Sanudo, 146.

[59] The fifteenth century witnessed increasing differentiation between sons of Great Council members and men whose fathers, though eligible for membership, had never formally proved their eligibility. This distinction, little noted in the literature, created an official stratification within the patriciate, strengthened the fusion between patrician status and active participation in government, and

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The significance of the Balla d'Oro extended beyond the youthful competitors and their immediate families as well as the solemn lottery of December 4th. The transition ritual actually began with registration in front of the *avogadori di comun*, at which a young man, establishing his legitimate patrician birth and producing evidence that he was eighteen years old, detached himself from youthful society. The procedure was fixed by the regulating act of 1414. Registrants were to be presented by their fathers or, in their absence, by near kinsmen, who were to swear to the *avogadori*, under penalty of fine, that the young men were indeed eighteen years old and legitimately born of patrician fathers.[60] The act says nothing about other participants, but two and sometimes three additional persons normally acted as pledges for payment of possible fines.[61] Although many of these guarantors belonged to the lineage of the candidate's father or mother, nearly two-thirds were members of other patrician houses, indicating that a widening circle of friends, neighbors, patrons, and clients were joining kinsmen in the ritual that officially solemnized the emergence of an adult noble.[62] By the time young patricians turned twenty, then, they had participated in two rituals detaching them from the ranks of adolescents, rituals that impressed on them the desirable dignity of political adulthood and deepened their awareness of and dependence on the community of elders, kinsmen, and others, who guided them into full adult patrician status. It was during the period of liminality inaugurated by these ritual moments that young men gave themselves over to boisterous episodes. The connection between these facts brings us closer to the role of young men in the patrician regime.

THE YOUTHFUL DISORDERS IN THE GREAT COUNCIL PROVOKED INDIGNANT retribution. But once the heat of the moment cooled, the mature majority of members followed a benign policy toward liminal youths, continuing to admit men in their early twenties to Council membership and, as noted, fully rehabilitating the disorderly among them. The patriciate, as a collective regime and as individual families, willingly endured bumptious youthful outbursts in order to inculcate in the young the centrality of politics for the ruling class, and, specifically, the disposition toward government service.[63] The youthful excesses exceeded in the

encouraged continuity from father to son in the sociopolitical strategies of families. I propose to treat this subject more fully in my monograph, "Public Manhood in Renaissance Venice," in preparation. For the special status of sons of Council members, see MC 23, Regina, f. 27v (January 28, 1459); Dieci, Miste 31, f. 101rv (August 19, 1506).

[60] MC 21, Leona, ff. 241v–242r.

[61] For the 755 registrations sponsored by either the candidate's father or mother, 1,342 guarantors participated. See Chojnacki, "Kinship Ties and Young Patricians," 256.

[62] Of the 1,342 guarantors studied, 873 (65 percent) came from non-parental lineages. For a general discussion of kin involvement in Barbarella registrations, see *ibid.* The guarantors from other lineages were likely tied to the families of registrants by bonds of patronage, possibly reinforced by spiritual kinship, whose strength is hinted at by an act of the Council of Ten in 1505 forbidding nobles to stand as godfathers (*copatres*) at the baptisms and confirmations of other nobles' children. Dieci, Miste 30, f. 191r.

[63] The family stake is apparent in measures enjoining fathers to control their sons' behavior: an act of 1443 made fathers liable for fines levied on youngsters cavorting in the Council chamber; MC 22, Ursa, f. 148v.

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right direction—toward the institutional politics centered in the Great Council, politics that gave the ruling class its distinctive identity and apportioned the advantages of membership in it. The toleration shown to unruly young members of the Great Council contrasts with the fierce reaction to the bloc-voting plot of 1433, as a comparison of penalties makes clear. Piero Priuli and the other youths who burst out of the chamber in 1442 were each fined fifty lire and deprived of Council membership for six months, the same penalty that fell on their counterparts of 1446, while those of 1460 were simply given the six-month suspension and the threat of a hundred-lire fine for each time they showed up during that period.[64] Though mild, the penalty of exclusion from Council business is an additional indication that Council membership had value for young men. The conspirators of 1433, in contrast, received sentences ranging from three-year suspensions of membership in the Great Council to outright banishment from Venice for ten years and perpetual loss of the right to hold office.[65] The difference is striking. Misbehavior in the Council chamber was offensive, even disgraceful. But it was far less heinous than electoral conspiracies by the young away from official scrutiny—away, that is, from the socializing arena of the Great Council, where the adult majority was able, for its various purposes public and private, to orchestrate the process of political coming-of-age.

Once having turned twenty-five, young men, whether or not they had been unruly, were quickly drawn into governmental responsibilities. In the great majority of cases studied, nobles in politics won their first elective posts before turning thirty.[66] Indeed, the careers of men under thirty give the impression that pent-up expectations propelled them into government service as soon as they had overcome the age barrier, with many men winning their first elections at twenty-five.[67] Just as the long, elaborate transition to adulthood began with the ritual of registration for the Balla d'Oro, so it ended with a formal ceremony at which the electee provided evidence, often in the form of testimony by other nobles, that he had reached office-holding age and could assume his new post.[68]

[64] Raspe, 3649/9, ff. 12rv, 135r; Raspe 3651/11, f. 81r.

[65] Dieci, Miste 11, ff. 64r–71v; "Cronaca Zancaruol," ff. 142v–143r.

[66] This information emerges from examination of the careers of 149 men from the sixteen sample houses who registered for the Balla d'Oro from 1467 to 1475. One hundred went on to hold government office; of these, sixty-nine got their first posts while still in their twenties; BO 164, Voci 6.

[67] Baldassare di Marco Lando, aged twenty at his Barbarella registration in 1451 (where it was stated that he was born in 1431), was elected to the financial office of the Misseteria in 1456. Domenico di Antonio Zane, registered for the Balla d'Oro, assertedly at age eighteen, in 1450, was elected a silver official in 1457; BO 163, ff. 194v, 45v; PE 169, ff. 3v, 23. Antonio di Fantin Arimondo, registered at eighteen in November 1480, was elected *podestà* at Noale in February 1487. BO 164, f. 2v; Voci 6, f. 30r.

[68] Electees usually proved their ages by reference to their Balla d'Oro registrations, on file after 1414 in the records of the Avogaria di Comun, but sometimes by testimony of relatives and friends, as in the Barbarella registrations. It is not clear, for example, why Andrea Navagero, elected a meat-market supervisor (*ufficiale alle beccarie*) in 1456, called on Nicolò Bondumier and Benedetto Gritti to testify that he was twenty-five instead of simply producing the record of his Balla d'Oro registration in early 1448. The likelihood is that by involving them in the proof of his office-holding credentials, Andrea was strengthening his relationship with them, which went back at least as far as 1448 in the case of Gritti, who had registered Andrea for the Barbarella. (I have been unable to establish ties of kinship between the Gritti and Navagero families.) BO 163, f. 274v; PE 169, f. 6r.

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These proofs of eligibility for office, like the Balla d'Oro registrations, were only ordered into careful procedures and recorded by the presiding officials in the early fifteenth century, another sign of the new concern with age, birth, and office that was giving a fixed shape to the political ideology of the nobility.[69]

Although these proofs marked the act of incorporation into formal political adulthood and the completion of the liminal period, they did not end age-based political restrictions. Robert Finlay has discussed in detail the higher age requirements for higher posts (also documented in the proof-of-age registers).[70] The presence of men older than twenty-five among the conspirators of 1433 and the disrupters of 1442, 1446, and 1460 shows that frustration over limits on the political opportunities for patricians under thirty could galvanize energies on both sides of the twenty-five-year-old age divide. Efforts of nobles over twenty-five to falsify their ages to gain higher office—sometimes by picturesquely devious ruses—also testify to frustrated aspirations for office lasting beyond the liminal period.[71]

A graduated liminality was built into the Venetian political system, as it is in all systems with different age requirements along the *cursus honorum*. The principal aims of the government in reserving certain posts for experienced office-holders were to ensure competence in officials and to direct office stipends to the fathers of families who had special need of them.[72] At the same time, by introducing men into office-holding while still denying them positions of prestige, power, or remuneration, the government kept alive an impatience that bore some resemblance to the rambunctious eagerness of the young men in the liminal period between Balla d'Oro registration and the first governmental post.

But eagerness for preferred posts is fundamentally different from restiveness over the complete denial of office-holding eligibility. Ambition among men qualified for office, even those between twenty-five and thirty, belongs more to the steady rumble of electioneering that lay at the heart of noble politics than to the

[69] PE 177, the first register in the series recording proofs of age, opens with the texts of a Great Council act of 1430 and a Senate act of 1431 requiring more careful official scrutiny of age and genealogical credentials for Council membership and office-holding. The acts, reproduced in the inside cover of the register, are MC 22, Ursa, f. 88r (March 5, 1430) and Senato, Misti 58, f. 77v (September 1, 1431). Another act revealing the heightened concern with credentials is one requiring the use of patronymics to identify precisely the candidates in elections to office. MC 21, Leona, f. 127rv (February 19, 1404).

[70] Finlay, "Venetian Republic as a Gerontocracy." The registers in the series beginning with PE 169 record proofs of age of Balla d'Oro winners and civil court advocates (who both had to be twenty), of Great Council entrants at age twenty-five, and of electees to offices with age requirements of twenty-five and thirty. The series beginning with PE 177 contains, in addition, age proofs for *patroni* of the merchant galley fleets (who had to be thirty) and for galley crossbowmen (who had to be twenty).

[71] In 1410, young Paolo Valier was so eager to take a seat on the Forty that he induced a woman to impersonate his mother and testify falsely—in his mother's bedchamber—that he had reached the required age of thirty; Raspe 3646/6, f. 89. Seats on the Forty were so ardently pursued by young men that many claimed to have reached the requisite age of thirty when only in their thirtieth year, calculating from conception rather than birth—which, said the Council of Ten in 1506, "absurdissimum est." The Ten therefore specified that age requirements for all offices were met only after the appropriate birthday had been reached ("etatem completam et non inceptam"). Dieci, Miste 31, ff. 122v–23r.

[72] There are repeated references in the fifteenth century to nobles' dependence on office. In 1490, the Great Council frankly acknowledged that it was the means by which "the majority live and support their families." MC 24, Stella, f. 109v.

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liminal cohort's enforced wait for office. Once having crossed the barrier to eligibility for election, members of politically ambitious or economically needy families were inevitably drawn into competing family strategies and factional maneuvering.[73] Although the exuberance of men in their late twenties gave a lively aspect to their contribution to family striving, and their exclusion from ducal elections reminded them of the limits on their role in the political process, once elected to their first office, they became part of the governmental structure and lost the sense of shared anomaly that united men still short of their twenty-fifth birthday, men who could participate in the political process by voting for officials but were denied the full political status of the men for whom they cast ballots.

The contrast between the solidarity that characterizes members of liminal groups and the divisions that run through adult society figures prominently in recent writing on rites of passage, notably that of Victor Turner. The essential trait of liminal groups, for Turner, is their lack of clear status, their ambiguous passage through "a cultural realm with few or none of the attributes of the past or coming state."[74] Though held back from full membership, neophytes are nevertheless expected to learn during the liminal period the principles of the mature state into which they will ultimately pass. Lacking a precise status and constantly reminded of their inferiority to their elders, liminal youths develop a strong sense of community, marked by "intense comradeship and egalitarianism."[75] Two qualities, internal solidarity and exclusion from full membership in the culture whose principles the youths are learning, are for Turner the basis of the liminal group's special character and function.

It is precisely the freedom of liminal youths from, in S. N. Eisenstadt's words, the "compromises inherent in daily participation in adult life" that, paradoxically, equips them to serve, as the "purest manifestation and repository of ultimate cultural and societal values."[76] To see how this pattern fits Venetian young men on the threshold, it is important to take note of the tendency of recent writers on patrician politics to view the patriciate as being divided on many levels.[77] On the level of daily life and elementary interests, competition for offices occupied

[73] Factional and familial jockeying is the chief theme of Finlay, *Politics in Renaissance Venice*.

[74] Turner, *Ritual Process*, 94.

[75] *Ibid.*, 95.

[76] S. N. Eisenstadt, "Archetypal Patterns of Youth," in Erik H. Erikson, ed., *The Challenge of Youth* (Garden City, N.Y., 1965), 29–50, esp. 32–33. Myerhoff also noted that "the liminal person comes to stand for . . . all that is universal, innate, whole, and unified"; "Rites of Passage: Process and Paradox," 117. Turner contrasted adult society and liminal youths as representing two different models of human relationships. The first is the "structured, differentiated, and often hierarchical system of political–legal–economic positions with many types of evaluation, separating men in terms of 'more' or 'less.'" The second type, "which emerges recognizably in the liminal period, is . . . society as an unstructured or rudimentarily structured and relatively undifferentiated *comitatus*, community, or even communion of equal individuals." *Ritual Process*, 96.

[77] Much traditional writing, focusing on the "myth" of Venetian governmental wisdom, has treated the patriciate as an undifferentiated bloc. More recently, the work of Cozzi, Cracco, Finlay, and Gilbert, among others, has traced divisions, rendering a more complex, nuanced picture of the patriciate acting as a cohesive group in some matters and dividing into factions, interest groups, and economic levels in others. For a useful formulation of the double character of social groups—outwardly unified, inwardly varied—see the discussion of "group and grid" in Mary Douglas, *Natural Symbols: Explorations in Cosmology* (2d edn., New York, 1982), 56–61.

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government councils throughout the fifteenth and sixteenth centuries. This competition led to a steady stream of acts aimed at eliminating fraudulent devices by which officials clung to their posts (and continued to draw their stipends) beyond their legal term, impeding the equitable distribution of offices throughout the class.[78] Recent scholarship has taken an increasingly sophisticated approach to the connection between this general desire for government posts and the maneuverings of interest groups and power blocs within the patriciate, stressing the crucial role of a restricted political and economic oligarchy at the center and the relations of its members with each other and with their clients among the less wealthy and influential nobles on the fringes.[79] The precise geology of these divisions remains to be plotted, especially the crucial question of whether the membership of the dominant group lasted over generations. An answer will require patient research of great ingenuity. But enough is now known to reject the old characterization of the patriciate as monolithic. In the present state of knowledge, it is not possible to gauge precisely the degree to which common liminality created solidarity among young men that might have overcome the divisions among their fathers. That measurement would require a comparison of the interactions of the young men with the relations among their elders in the adult patriciate, a task impossible at present because of the lack of systematic prosopographical research that could reveal which families were powerful and rich, which were needy and ripe for clientism or manipulation.[80]

But there is one test that can be applied, a test based on distinctions of antiquity, which asks whether young members of Venice's oldest noble houses, the *case vecchie*, mingled in liminal fellowship with their contemporaries from houses of more recent nobility, the *case nuove*. In the fifteenth century, these two groups were engaged in a tense, sometimes conflict-marked struggle over preeminence in the government, symbolized by the dogeship.[81] The importance of these alignments should not be overstated; they did not carve a deep fissure through the patriciate. Like other intra-class distinctions, they marked off for certain issues antagonists who consorted amicably in other equally important contexts.[82] Yet, since the

[78] MC 23, Regina, ff. 7v–8r (July 13, 1455); MC 24, Stella, f. 109v (September 21, 1490).

[79] Finlay, *Politics in Renaissance Venice*, 59–81; Giorgio Cracco, "Patriziato e oligarchia a Venezia nel Tre-Quattrocento," in Sergio Bertelli, *et al.*, eds., *Florence and Venice: Comparisons and Relations*, 2 vols. (Florence, 1979), 1: 71–98. For other recent discussions of differences within the class, see Chojnacki, "In Search of the Venetian Patriciate"; Cozzi, "Authority and the Law in Renaissance Venice"; Cracco, *Società e stato nel medioevo veneziano*; Gilbert, "Venice in the Crisis of the League of Cambrai"; and Jean-Claude Hocquet, *Le Sel et la fortune de Venise*, vol. 2: *Voiliers et commerce en Méditerranée* (Villeneuve-d'Ascq, 1979).

[80] Discussing Florence in the same period, Anthony Molho has observed that the emphasis of recent historians on prosopography has obscured elements of class conflict. "Cosimo de' Medici: *Pater Patriae* or *Padrino*?" *Stanford Italian Review*, 1 (1979): 9. His words also have cautionary value for Venice, but assessment of the social and factional components of Venetian political history requires careful reconstruction of the membership of these groups in order to scrape away the effects of the long tradition of generalization about the undifferentiated patrician regime.

[81] On the *case vecchie* and *case nuove* (also referred to as the *longhi* and the *curti*), see above, note 11. But note the corrective comments of Merores, "Der grosse Rat von Venedig," 64–70.

[82] In the important area of matrimonial choice, the *case vecchie* (among the sixteen-clan sample studied) chose 44.4 percent of their children's spouses from outside their own numbers. Marco Barbaro, "Libro di nozze patrizie," BMV, Codici Italiani, classe VII, 156 (=8492).

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distinction between the *case vecchie* and the *case nuove* (or, alternatively, between the *longhi* and the *curti*) was one recognized by contemporaries as a source of tension within the class, it permits a test of the communitarian spirit of liminal young men. The test reveals that disrupters of the Great Council, office-seeking conspirators, and even members of the youthful social fraternities known as the *compagnie della calza* came from all points on the genealogical spectrum, from the most ancient of noble families to those accorded patrician status as recently as 1381.[83]

Young patricians, forbidden to trespass onto the adult battlefields on which were mobilized snobberies, resentments, and conflicting interests, were able to find in each other's company, if not solidarity, at least a fellowship of youthful patterns of behavior with which they responded collectively to the restrictions placed on their adulthood—restrictions based not on antiquity, wealth, or family prominence, but on the age they had in common. Indeed, it is tempting to see in the restrictions of the threshold period a desire on the part of the elders to keep the young unsullied by the conflicts that animated the patriciate. But, equally likely, limitations reflected an adult consensus that young men, if permitted active participation in the political give-and-take, threatened to go beyond acceptable limits of political activity—to organize office-seeking cabals such as that of 1433.[84] Young men needed the socialization into patrician politics acquired in a period "on the threshold," observing the process, competing for prizes such as the Barbarella victory, swallowing disappointment at missing them and at having to wait still longer, even—like Piero Priuli and his comrades—taking out their frustrations in boisterous antics in the Council chamber. They needed, in short, a period of directing their youthful energies toward the increasingly clearly focused public and private vocations of patrician politics.

The role of the young in the political culture of the patriciate encompassed more than the socializing transition to political manhood. As protagonists of the ritual process that elucidated the stages and nature of patrician adulthood, young men served as symbolic embodiments of the patriciate's evolving definition of itself as a ruling class. Rivalries and antagonisms continued, inevitably, to cause tensions

[83] The Great Council fugitives of 1442 included members of the Barozzi, Bembo, and Contarini clans, all *case vecchie*, but also Nicolò Paruta, member of a family ennobled in 1381. In the episode of 1446, young men bearing such ancient names as Pisani, Zane, and Loredan were implicated, but so were Francesco Lippomano and Girolamo Vendramin, sons of families ennobled in 1381. Raspe 3649/9, pt. 1, f. 7v; pt. 2, f. 14r. The electoral conspirators of 1433 included members of the oldest houses such as Giovanni Soranzo and Zaccaria Contarini but also Marco Cicogna, of the inductees of 1381; "Cronaca Zancaruol," f. 142v. For the thirty men ennobled with their families after the War of Chioggia in 1381, see *Raphayni de Caresinis chronica*, ed. Ester Pastorello, *Rerum italicarum scriptores*, 2d edn., vol. 12, pt. 2, fasc. 1–2 (Bologna, 1923): 57. See also Chojnacki, "In Search of the Venetian Patriciate," 57. The signatories of the statutes of the *calza* company of the Modesti, drawn up on February 20, 1487 (the year, that is, after a tense struggle between *vecchi* and *nuovi* over the dogeship), included *vecchie*—Badoer, Bembo, Bragadin, Zorzi—and *nuove* families—Trevisan, Venier, Loredan, Malipiero. Biblioteca Correr, Codici Cicogna, 3278, fasc. 24, f. 29. On the events of 1486, see Romanin, *La storia documentata di Venezia*, 4: 305–06. On the *compagnie della calza*, see Venturi, "Le Compagnie della Calza."

[84] Writers on liminality have noted the tension between the "creativity and innovation" characteristic of neophytes in the liminal state and adult society's view of this freedom as "dangerous and anarchical, and [having] to be hedged around with prescriptions, prohibitions and conditions." For the statement on creativity, see Myerhoff, "Rites of Passage: Process and Paradox," 117; for that on danger and anarchy, see Turner, *Ritual Process*, 109.

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within the fifteenth-century patriciate, but they faded in the light of the newly articulated, collective commitment of the class to preserving an exclusive hold on the political vocation, with its duties and benefits. The tests that the young men of the fifteenth-century patriciate were obliged to pass, the restrictions that they had to endure, the prizes now more systematically bestowed on them, and the satisfying conclusion of their long transition to full noble status expressed for the patriciate a clear, new, and precise definition of itself.

THE TWO ELEMENTS AT THE CORE OF PATRICIAN STATUS IN THE FIFTEENTH CENTURY were exclusivism and public office—public office to satisfy practical needs both governmental and private, exclusivism to prevent diffusion of the benefits of office and to make enjoyment of them the prerogative of a genealogically specific caste. All these aims found institutional expression in the lengthy process by which male patricians passed from youth to adulthood. It was by means of the increasingly elaborate set of rituals that began, enlivened, and ended the liminal period that men established their identity as nobles and gained the political orientation essential to that identity. In articulating the steps to patrician adulthood, the rituals crafted by legislation after 1400 gave the patriciate as a whole its first opportunity to state its ideology. The punishments meted out to Piero Priuli and the other young disturbers of the Council and their swift rehabilitation into patrician political life can be seen as opportunities provided by the peculiar impulses of liminal youths for the patriciate to assert the dimensions of its status. Decorum, respect for officialdom, bowing to procedure, observing the limitations on different age levels, and, above all, blending individual interest with the collective purpose—these now emerged as the elements of a patrician ideology that served both public and private needs. A breach of these rules had to be punished. But the punishment itself and the necessity to impose it clarified, for the offending youths and offended adults alike, the principles flouted. At its highest levels, the patrician regime was indeed a gerontocracy, as Robert Finlay has argued. Piero Priuli's election at age sixty-one as a procurator of St. Mark exemplifies the predilection for steadiness, gravity, and maturity that characterized Venetian government.[85] But from the perspective of the development of the patrician regime, Piero's contribution to Venetian government was not limited to his successful conformity to the ideal of mature steadiness. It had its paradoxical beginning forty years earlier, when Piero and his young companions, in their rowdy fellowship, gave vivid focus to the role of liminal patricians in shaping the political ideology of Renaissance Venice.

[85] Finlay also observes that restricting high position to men of mature years encouraged shared values at the center of power by grouping there men who shared the experience of directing Venetian government and defining its goals. "The Venetian Republic as a Gerontocracy," 174.

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A Transatlantic Community of Saints: The Great Awakening and the First Evangelical Network, 1735–1755

SUSAN O'BRIEN

ON NOVEMBER 8, 1742, SEVERAL HUNDRED MEN AND WOMEN packed into the London Spa Fields Tabernacle to celebrate the latest news of religious revival. The high point of the meeting came when letters from all over the revival world were read out loud, after which the congregation joined in a hymn specially written for the occasion:

> Great things in England, Wales and Scotland wrought,
> And in America to pass are brought,
> Awaken'd souls, warn'd of the wrath to come
> In Numbers flee to Jesus as their Home. . . .
> What is this News, that flies throughout our Land?[1]

This homespun hymn serves as a reminder that the events of the late 1730s and 1740s that historians have analyzed separately and severally as the American Great Awakening, the English Evangelical Revival, and the Scottish Cambuslang Wark, were perceived by many participants as parts of a single God-inspired phenomenon. Given the direction of the historiography during the past twenty years, the reminder is both timely and necessary.

The religious revivals of the period 1735 to 1750 have attracted the attention of an impressive number of historians, but few have made more than passing reference to their broad appeal. Rather, as the historical literature developed, the focus of inquiry narrowed from national and denominational levels of analysis to detailed studies of the regional and local settings. As a result, historians have gained new insights into the nature, causes, and meaning of the revivals, but our understanding of a revival in one country has remained isolated from our

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[1] *Weekly History* (London), November 13, 1742, no. 84.

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knowledge of a revival elsewhere.[2] This point can best be illustrated by reference to the most developed revival historiography—that on the American Great Awakening. Throughout the 1960s and 70s, scholars augmented the existing denominational and regional studies by a number of local studies, each seeking to bring out those characteristics of a particular environment that made it open to the revival impulse.[3] By the late 1970s, the weight of research and interpretation caused American historians to regard the Awakening as a peculiarly American phenomenon, central to the formation of a religious tradition whose hallmarks were millennial fervor, evangelicalism, and periodic schism.[4] Following this line of thought, some scholars described this very tradition as a distinguishing feature of American culture, and such an assessment, as Jon Butler recently pointed out, has become entrenched in American history survey texts.[5] Butler's critique of American "enthusiasm" for the Great Awakening, though contested, may well mark a turning point in our understanding. His stimulating essay argues that the Great Awakening was neither "great nor general," and its significance therefore cannot be as far reaching as historians have believed. Central to his case is the argument that American revival studies have unjustifiably centered on New England. Revivalism, in Butler's view, was both more than the Calvinist revival of New England and less than "the Key to the American Revolution."[6] Another look at the evidence suggests that a more serious recognition of revivals outside America could have provided Butler with a further angle from which to question the "American-ness" of the Awakening. This missed opportunity is characteristic of the historiography.

Given the predisposition to view the Great Awakening as American, the relatively slight interest in the comparative history of revivalism or in the possible connections and influences among revivalists is hardly surprising. For different but

[2] J. M. Bumsted used the phrase "splendid isolation" to describe the historiography of the Great Awakening. See Bumsted, "'What must I do to be saved?' A Consideration of Recent Writings on the Great Awakening in Colonial America," *Canadian Association for American Studies Bulletin*, 4 (1969): 50.

[3] See, for example, J. M. Bumsted, "Presbyterianism in Eighteenth Century Massachusetts: The Formation of a Church at Easton, 1752," *Journal of Presbyterian History*, 46 (1968): 243–53, and "Revivalism and Separatism in New England: The First Society of Norwich, Connecticut, as a Case Study," *William and Mary Quarterly*, 3d ser., 24 (1967): 588–612; Gerald F. Moran, "Conditions of Religious Conversion in the First Society of Norwich, Connecticut, 1718–1744," *Journal of Social History*, 5 (1972): 331–43; James Walsh, "The Great Awakening in the First Congregational Church of Woodbury, Connecticut," *William and Mary Quarterly*, 3d ser., 28 (1971): 543–62; and W. F. Willingham, "Religious Conversion in the Second Society of Windham, Connecticut, 1723–1743; A Case Study," *Societas*, 6 (1976): 109–19.

[4] For an expression of this position, see, among others, Sydney E. Ahlstrom, *A Religious History of the American People* (London, 1972): 280–94; and Timothy L. Smith, "Congregation, State and Denomination: The Formation of the American Religious Structure," *William and Mary Quarterly*, 3d ser., 25 (1968): 155–76.

[5] Jon Butler, "Enthusiasm Described and Decried: The Great Awakening as Interpretive Fiction," *Journal of American History*, 69 (1982): 305–25.

[6] William G. McLoughlin, "'Enthusiasm for Liberty': The Great Awakening as the Key to the Revolution," in Jack P. Greene and William G. McLoughlin, eds., *Preachers and Politicians: Two Essays on the Origins of the American Revolution* (Worcester, Mass., 1977), 47–73. See also Alan Heimert, *Religion and the American Mind: From the Great Awakening to the Revolution* (Cambridge, Mass., 1966); Cedric B. Cowing, *The Great Awakening and the American Revolution* (Chicago, 1971); and Harry S. Stout, "Religion, Communications, and the Ideological Origins of the American Revolution," *William and Mary Quarterly*, 3d ser., 34 (1977): 519–41.

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equally intelligible reasons, scholars have also viewed the history of the English revival in isolation.[7] In its broadest sense, revivalism in the mid-eighteenth century not only appeared in several places and involved a range of theologies and religious polities, including Anglican, Lutheran, and Pietist, as well as Calvinist, but these groupings also shared connections and influences.[8] Evidence suggests that Calvinist evangelicals on both sides of the Atlantic were highly conscious of one another's activities. This transatlantic evangelical consciousness grew out of the isolated correspondence of individual ministers. Ministers and lay promoters extended the correspondence into a reliable, nonpersonal system of contacts, which they developed into a number of procedures for spreading the news from individuals to groups of committed laity and beyond to a wider lay audience. Because revival news was of great importance to Calvinist evangelicals, they had a strong motivation to create a relatively durable chain of correspondence. Once in place, the contacts proved useful to the ministers in other ways. Some uses were practical, for example, the circulation of devotional literature, recommendations for suitable reading, the collection of money for missionary work, and the provision of hospitality. On another level, ministers used transatlantic contacts for the discussion of theological questions, the nature of piety, and the practice of revivalism. It is not too much to say that through the exchange of ideas and materials Calvinist revivalists of the mid-eighteenth century built a "community of saints" that cut across physical barriers and, on occasion, theological divisions.

The significance of this community lies not in its novelty—there was nothing new about associations of ministers, or religious communication systems, even transatlantic ones—but rather in its effect on our understanding of eighteenth and

[7] The religious history of mid-eighteenth-century England has been written almost exclusively by church historians and, because of the success of Wesleyanism, the Methodist perspective on the English revival has tended to swamp all others. Such concentration helps to explain the lack of attention paid to the transatlantic dimension of revivalism in this period, because the exchanges of ideas and literature did not involve Wesley or his followers. For a recognition of the interconnections between Whitefieldians, Wesleyans, and Dissenters in the 1740s, see Richard W. Evans, "The 18th Century Welsh Awakening with Its Relationship to the Contemporary English Evangelical Revival" (Ph.D. dissertation, Edinburgh University, 1955); Geoffrey Nuttall, *Philip Doddridge 1702–1751: His Contribution to English Religion* (London, 1951); John D. Walsh, "Origins of the Evangelical Revival," in Gareth V. Bennett and John D. Walsh, eds., *Essays in Modern English Church History* (London, 1966) and "The Cambridge Methodists," in Peter Brooks, ed., *Christian Spirituality: Essays in Honour of Gordon Rupp* (London, 1975), 251–83; Charles E. Watson, "Whitefield and Congregationalism," *Transactions of the Congregational Historical Society*, 8 (1920–23): 171–80, 273–345; Michael R. Watts, *The Dissenters: From the Reformation to the French Revolution* (Oxford, 1978), chap. 5.

[8] On connections involving pietists, for example, see Milton J. Coalter, "The Radical Pietism of Count Nicholas Zinzendorf as a Conservative Influence on the Awakener, Gilbert Tennent," *Church History*, 49 (1980): 35–46; John B. Frantz, "The Awakening of Religion among the German Settlers in the Middle Colonies," *William and Mary Quarterly*, 3d ser., 33 (1976): 266–88; Gillian L. Gollin, *Moravians in Two Worlds: A Study of Changing Communities* (New York, 1967); James E. Hutton, "The Moravian Contribution to the Evangelical Revival in England 1742–1745," in Thomas F. Tait and James Tout, eds., *Historical Essays* (Manchester, 1907); F. Ernest Stoeffler, *The Rise of Evangelical Pietism*, Studies in the History of Religions, 9 (Leiden, 1965); W. R. Ward, "The Relations of Enlightenment and Religious Revival in Central Europe and in the English Speaking World," in Derek Baker, ed., *Reform and Reformation: England and the Continent c.1500–c.1750* (Oxford, 1979); and "Power and Piety: the Origins of Religious Revival in the Early Eighteenth Century," *Bulletin of the John Rylands University Library of Manchester*, 63 (1980): 231–52.

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nineteenth-century revivalism.[9] Although the historical literature is not devoid of references to "influence" and "cross-fertilization" between Britain, Northwest Europe, and the American colonies, recognition of these connections usually appeared in only two forms, neither of which has had much impact on interpretation.[10] In one, the focus of attention is George Whitefield, whose travels from England to Scotland and America are frequently mentioned. Scholars have traced revival connections between America and Britain and, in the process, reduced them to the activities of this wholly exceptional preacher.[11] The other type of recognition is of a different order. Historians working within a committed evangelical tradition have written of the revival much as the Spa Fields congregation saw it—as a widespread "stirring of dry bones," a movement of the Holy Spirit. Starting in 1754 with John Gillies' two-volume collection, Calvinist evangelical commentators have interpreted the eighteenth-century revival as broad and sweeping, careless of national and church boundaries, and evangelical in character.[12] Because of their commitment to a God-inspired explanation, historians in the evangelical tradition have not carefully examined the human causes and agencies of connection and influence and consequently have had little influence on secular historians. But the perception of the revival as international and evangelical is instructive, and evidence adduced by evangelical writers establishes

[9] There was a Puritan transatlantic network in existence between 1620 and 1730. As Cotton Mather wrote, "When the distance of the huge Atlantic separates Brethren from one another, one Method unto which we must resort for Maintaining the communion of saints is the epistolary." Carl Bridenbaugh, *Mitre and Sceptre: Transatlantic Faiths, Ideas, Personalities, and Politics, 1689–1775* (New York, 1962), 86. Also see Darrett B. Rutman, *American Puritanism: Faith and Practice* (New York, 1970), 16–21; and George Selement, *Keepers of the Vineyard: The Puritan Ministry and Collective Culture in Colonial New England* (Lanham, Md., 1984), 70. The Quakers developed their own transatlantic community in the early eighteenth century. See Frederick B. Tolles, "The Atlantic Community of the Early Friends," *Journal of the Friends Historical Society*, Supplement no. 24 (1952); and *Quakers and the Atlantic Culture* (New York, 1960).

[10] For a recognition of the transatlantic dimension, see John W. Raimo, "Spiritual Harvest: The Anglo-American Revival in Boston, Mass. and Bristol, England, 1735–1742" (Ph.D. dissertation, University of Wisconsin, 1974); and Watts, *Dissenters*, 394–406. Neither author, however, includes the Scottish revivalists, who were particularly active in the transatlantic network. In his discussion of enlightenment and religious revival, Ward did refer to "an international Calvinist network at the centre of which were ministers of the Church of Scotland," but it was not his concern to elaborate on the point. Ward, "Relations of Enlightenment and Religious Revival," 292.

[11] This is a position adopted, for example, by one of Whitefield's more recent biographers. Arnold Dallimore states that "Whitefield's ministry was the one human factor which bound this work together in the lands it reached." See Dallimore, *George Whitefield: The Life and Times of the Great Evangelist of the 18th Century Revival*, 2 vols. (London, 1970), 1: 14.

[12] John Gillies, *Historical Collections relating to remarkable periods of the success of the Gospel, and eminent Instruments employed in promoting it*, 2 vols. (Glasgow, 1754). The two volumes were revised and enlarged in 1845 by Horatius Bonar and a facsimile of this edition has been published (Fairfield, Pa., 1981). In addition to the *Historical Collections*, Gillies edited an *Appendix to the Historical Collections* . . . (Glasgow, 1761) and *A Supplement to the Historical Collections* . . . (Glasgow, 1796). Later Scottish evangelical historians writing in this tradition include John W. Couper, *Scottish Revivals* (Dundee, 1918); Duncan MacFarlan, *The Revivals of the Eighteenth Century, particularly at Cambuslang* (Edinburgh, 1847); and John MacInnes, *The Evangelical Movements in the Highlands of Scotland, 1688–1800* (Aberdeen, 1951). An important American text in the same tradition is Joseph Tracy, *The Great Awakening: A History of the Revival in the Time of Edwards and Whitefield* (Boston, 1842). More recently, but in the same tradition, Archibald Fawcett recognized the ties between Scotland and America. Even so, he concluded, "it is not easy to trace any definite connection between these movements and the little community at Cambuslang, and there is something infectious about such happenings." Fawcett, *The Cambuslang Revival: The Scottish Evangelical Revival of the Eighteenth Century* (London, 1971), 53.

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that revivalists of the 1740s discussed the devices and methods for promoting and sustaining their work with colleagues all over the world. Indeed, their exchanges created an evangelical communications network long before the more widely recognized networks of the nineteenth century.

The major contrast that historians have drawn between the spontaneity of the mid-eighteenth-century revivals and the professionalism of those in the nineteenth century is misleading.[13] Instead, the eighteenth-century revivals should take their place on a continuum of Protestant evangelical development, with its starting point in the seventeenth century. The real significance of the mid-eighteenth-century revivals was not their wondrous spontaneity or their primary role in the formation of national consciousness, but rather their combining of traditional Puritan practices with fresh evangelical techniques and attitudes. It was a combination that played a major part, and perhaps the most creative part, in the systematic development of evangelicalism.

In 1736, news of the recent revivals in Hampshire County, Massachusetts, crossed over to England in the letters of Boston minister Benjamin Colman to London Dissenting ministers John Guyse and Isaac Watts. These same men encouraged Jonathan Edwards to write an account of the awakening for publication. This was to become Edwards's famous *Narrative*, first published in London.[14] The correspondence between Colman, Watts, and Guyse that made this possible exemplified the Old Dissenting network—a network with its roots in the seventeenth-century Puritan "community of saints." By the mid-eighteenth century, Dissenters had not only developed informal contacts across the Atlantic, but had also established a formal machinery, the Dissenting Deputies, to protect their civil and legal welfare.[15] It would be misleading, therefore, to underestimate the extent of communication already in existence before the evangelical revival. Colman and Watts, for example, corresponded and exchanged books and pamphlets from the 1720s until a few months before Colman's death in 1747. Watts carried on an extensive correspondence with other New England religious leaders, including Samuel Mather, Thomas Prince, Sr., and Elisha Williams. Through his generous gifts of books to Harvard, Watts demonstrated his interest

[13] For a classic expression of this distinction, see Calvin Colton, *The History and Character of American Revivals of Religion* (London, 1832). Most evangelical histories echo his view of eighteenth-century revivals. See also William G. McLoughlin, *Modern Revivalism* (New York, 1959), 11: "The difference between Edwards and Finney is essentially between the medieval and the modern tenure." This statement has been taken up more recently by John Kent in his discussion of the American revivalist tradition. See Kent, *Holding the Fort: Studies in Victorian Revivalism* (London, 1978), 24 and chap. 1.

[14] Jonathan Edwards, *A Faithful Narrative of the Surprizing Work of God in the Conversion of Many Hundred Souls in Northampton, and the Neighbouring Towns and Villages of New-Hampshire in New England in a letter to the Revd. Dr. Benjamin Colman of Boston. With a large preface by Dr. Watts and Dr. Guyse* (London, 1737). The exchange of correspondence involved in this first printing of the *Narrative* has been fully traced. See Clarence C. Goen, *The Great Awakening*, vol. 4 of *The Works of Jonathan Edwards*, John E. Smith, gen. ed., (New Haven and London, 1972), 32–46.

[15] Maurice W. Armstrong, "Dissenting Deputies and the American Colonies," *Church History*, 29 (1960): 298–320; Carl Bridenbaugh, *Mitre and Sceptre*, 44; and Bernard L. Manning, *The Protestant Dissenting Deputies* (Cambridge, 1952).

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in and concern for the Dissenting community abroad.[16] When the New England ministers wanted an English sponsor for Edwards's revival narrative, Watts was the obvious choice. But the colonial ministers were mistaken if they believed that the support they received from Watts, or from another interested correspondent, Philip Doddridge of Northampton (England), was similar to the response they could expect from English Dissenters.[17] Watts and Doddridge were attempting to occupy a middle ground between antinomianism and Arminianism and, as a result, remained relatively isolated among English Dissenters. Although colonial promoters of the revival narrative turned naturally to these two men, and, although both took part in the revival exchanges, they were not representative of English Dissent and of its response to the aims and style of revivalist religion.

For political, organizational, and theological reasons, the official channels of Dissent were not open to the revival of religion when it happened. Many Dissenters distrusted the tenets that became associated with revivalism: the emotionalism and even dogmatism of some of the converted, the new relationship proposed between clergy and laity, and the notion that an inspired preacher might be more efficacious than a trained minister would be. News of the New England conversions threatened to bring confrontation within both Nonconformism and Presbyterianism. The majority of English Nonconformist ministers, along with the spokesmen of the Assembly of the Church of Scotland, and of the Associate Presbytery, ultimately found they could not support the evangelicals. As a result, they lost their primacy in the transmission of revival news to a new grouping. The focus shifted from them to the Calvinist Methodists in England, to an active group of revivalists in the Church of Scotland, and to their kindred spirits in New England and the Middle Colonies. Because Anglican evangelicals, not Nonconformists, led the English revival, and because groups in Scotland, Wales, Holland, and America shared in the revival, a new religious community based on Calvinist evangelicalism came into existence.

This evangelical community was established through a network of correspondence that had George Whitefield at its center. Between 1739 and his death in 1770, in addition to his extensive preaching tours of England and Wales, Whitefield made seven journeys to America and fourteen to Scotland. His willingness to travel was only the most obvious expression of his eagerness to see the revival world as a single entity and to encourage others to do the same. His correspondence was as continuous and wide-ranging as his travels, and although we have no accurate figure of the number of people Whitefield wrote to, some impressions of the scale of his correspondence can be gained from his published letters and journals. He wrote regularly to all the main revival figures in Britain

[16] Massachusetts Historical Society (hereafter, MHS), Benjamin Colman Papers; "Letters of Dr. Watts," *Proceedings of the Massachusetts Historical Society*, 2d ser., 9 (Boston, 1895), 331–410; Thomas Milner, *The Life, Times and Correspondence of Rev. Isaac Watts, D.D.* (London, 1834); and Anne S. Pratt, *Isaac Watts and his Gifts of Books to Yale College* (New Haven, 1938).

[17] Nuttall, *Philip Doddridge*. Dr. Williams's Library, London, has the Doddridge MSS collection, and a calendar of his letters has been prepared. See Geoffrey F. Nuttall, *Calendar of the Correspondence of Philip Doddridge D.D.* (London, 1979), and John D. Humphreys, *The Correspondence and Diary of Philip Doddridge*, 5 vols. (London, 1829–31).

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and America and to hundreds of other ministers and lay people. Gillies published nearly 1,500 of his letters in the early 1770s, and the evangelical magazines of the 1740s included many items from his correspondence. One unpublished collection contains letters from over fifty different correspondents.[18] When Whitefield sent letters from the colonies to London for distribution, he did so by the trunkload. He received so many letters himself that he was glad to accept the secretarial help of lay evangelicals, such as stockbroker William Seward and teacher John Syms, although the replies were nearly always Whitefield's own.[19] American correspondents received instructions through the *Pennsylvania Gazette* "to direct their letters [for Whitefield] to be left with Mr. James Hutton, Bookseller without Temple, London."[20] Many who wrote were ordinary men and women who sought spiritual advice from Whitefield, thanked him for preaching, or gave him local revival news. Their letters reflect their intense personal experiences and the importance of Whitefield as a focal point in the revival world. His centrality explains the exclusive emphasis that historians have placed on him, but Whitefield did not work alone, and the international dimension of the revival would not have had the same significance had it relied solely on the activities of one individual.

Within just seven years of the Northampton revival, an international epistolary circuit had developed. Although the emergence of the circuit owed a great deal to the common focus evangelicals had in Whitefield, it owed just as much to the meaning that many participants invested in the revival, and particularly in its widespread occurrence. Jonathan Edwards reflected on this meaning in a letter to James Robe of Edinburgh: "The Church of God, in all Parts of the World, is but one; the distant members are closely united in one Glorious Head. This Union is very much her Beauty; [as is] the mutual and friendly Correspondence of the various members in distant parts of the world."[21] Others perceived the conjunction of the Northampton revival with the conversions of Howell Harris, George Whitefield, and the Wesleys, as heralding a special work of God, perhaps even the beginning of the millenium. "The Lord seems to have some great Event upon the Wheel just now," wrote Hugh Kennedy of the Scottish Church in Rotterdam, "and I would fain hope, the Glory of the Latter days is not far off. The present convulsions and reelings among the nations, as well as the stirring of Dry Bones in Scotland, America and other places, confirms me more and more in this

[18] *George Whitefield's Journals* (London, 1960); John Gillies, ed., *Works of George Whitefield*, 6 vols. (London, 1771–72), vols. 1 and 2. The unpublished collection cited here is Library of Congress MY–1–314, "Original Letters from various Persons in Great Britain and America to the Rev. George Whitefield from the Year 1738 to 1769. Collected by Thomas Raffles, Liverpool."

[19] William Seward (1711–40) described himself as a "Gentleman, Companion in Travel to the Reverend Mr. Whitefield." See Dallimore, *George Whitefield*, 1: 252. He had been brought up on the estate of a country squire near Bristol, where his father was a private secretary and had gone into stockbroking. Until his death, Seward was Whitefield's financial backer. See *Journal of the Calvinistic Methodist Historical Society*, 25 (1940), and 58 (1973). John Syms worked at the London Tabernacle where, at one time and another, he taught in the boys' school, dispatched letters, and kept the accounts for Whitefield's books. See Edwin Welch, ed., *Two Calvinistic Methodist Chapels 1743–1811: The London Tabernacle and Spa Fields Chapel* (London, 1975), 1: 5–7, 12; and Gomer M. Roberts, *Selected Trevecka Letters 1742–1747*, Trevecka Records Series no. 1 (Caernarvon, 1956), 215.

[20] *Pennsylvania Gazette*, December 4, 1740.

[21] James Robe, ed., *Christian Monthly History* (Edinburgh, 1745), vol. 2, no. 8, p. 235.

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opinion."[22] Spurred on by this sense of the unique times in which they were living, ministers began to correspond with people they did not plan to visit. Initially, the desire to acknowledge and share a common experience was sufficient to prompt a letter between strangers. Ministers saw themselves as co-workers and "friends in God." They found one another's names in revival publications, Whitefield's published journals, the religious press, or through a friend: "It was a most pleasing surprise to me to receive your kind letter from New York," Colman wrote to Whitefield, "and the valuable Present of Journals, Sermons and Letters which accompanied it. I think myself under happy Direction of Providence in my writing to Mr. Pemberton since it has brought me into correspondence with you."[23] It was common to solicit a correspondence by referring to a mutual friend or a published work by the recipient. For example, Thomas Gillespie of Carnock in Scotland recommended himself to Edwards by praising Edwards's works: "The two performances you published on the subject of the late glorious work in New England, well adapted to that in Scotland, gave me great satisfaction. . . . I have many a time, for some years, designed to claim humbly the privilege of correspondence with you. . . . My friend and countryman Robert Abercrombie will inform you about me, if you have occasion to see him or hear from him."[24] In the case of John Hamilton of the Barony Parish in Glasgow, contact was made through a traveler and mutual friend: "This letter, I trust shall be deliver'd to you," he wrote to Thomas Prince, Sr., "by our Brother Mr. George Brown, Minister of St. James Island in South Carolina . . . , who is to go by way of Boston; which gives me the opportunity of sending my compliments to you and of introducing myself into your Acquaintance and favour."[25] Even printers were drawn into the epistolary circuit, for example, John Lewis of London, who worked for the revivalists, and whose print shop was often a useful clearing house for correspondence and revival literature.[26] Lewis felt honored to be recognized by Thomas Prince: "Who could ever imagine that such a poor insignificant creature as little I should ever have been thought of so many thousands of miles off. . . . I received your kind letter and . . . I shall be very glad to have a correspondence with you."[27]

[22] William McCulloch, ed., *Glasgow Weekly History* (Glasgow, 1742), no. 43, p. 1. For another contemporary millennial view, see John Erskine, *The Signs of the Times Consider'd, or the high Probability that the present Appearances in New England, and the West of Scotland are a prelude of the Glorious Things promised to the Church in the Latter Ages* (Edinburgh, 1742).

[23] Colman to Whitefield, 1742, MHS, Benjamin Colman Papers. This letter has been dated 1742, but it is more likely to have been 1739 since it was printed in 1739 with two other of Whitefield's letters. George Whitefield, *Letter from New Brunswick and Letter in Answer to the Presbytery of Newcastle* (Philadelphia, 1739). Whitefield corresponded with Colman before 1742. See *Whitefield's Journals*, 457. He refers to Ebenezer Pemberton, at the time pastor of Wall Street Presbyterian Church, New York City.

[24] Gillespie to Edwards, November 24, 1747, in Sereno E. Dwight, *Life of President Edwards* (New York, 1830), 244. This is volume 1 of Dwight's ten-volume edition of Edwards's *Works*.

[25] Hamilton to Prince, Sr., March 3, 1739, MHS, Miscellaneous MSS Collection.

[26] John Lewis, who was originally from Monmouthshire, was a member of the London Tabernacle and the official printer to the Calvinist Methodists. See *Journal of Calvinist Methodist Historical Society*, 4 (1919): 84–92 and 5 (1920): 6–11.

[27] Lewis to Prince, Sr., August 20, 1743, MHS, Davis Papers. Dutton was an evangelical Baptist minister in Huntingdonshire, England, and the husband of writer and evangelical, Anne Dutton. See Stephen J. Stein, "A Note on Anne Dutton, Eighteenth Century Evangelical," *Church History*, 44 (1975): 485–91.

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These individual initiatives created a letter-writing network that had a core of ten leading ministers—Edwards, Colman, and Thomas Prince, Sr., of New England; James Robe, William McCulloch, John M'Laurin, and John Erskine of Scotland; and English ministers Watts, Whitefield, and Doddridge.[28] Their interlocking and overlapping connections made them a close-knit group. Whitefield was in contact with every member of this group, as was Edwards, while the rest corresponded with at least seven others. Colman, Watts, Doddridge, and Prince were simply continuing and extending their existing practice of communicating with like-minded ministers abroad. Prince, for example, was too elderly and infirm to take an active part in preaching the American revival, but he was able to promote it and lend it his authority through his correspondence. In contrast to these "old hands," none of the Scottish ministers, with the exception of John M'Laurin, had initiated long-distance communications before the 1740s, but they soon became regular and conscientious correspondents. John Erskine, for example, visited Cambuslang as an undergraduate, and after talking to revival ministers, he actively pursued contacts in England, Scotland, and America. He was a young man of independent means who did not hesitate to use his wealth to supply books free of charge and to pay postage if he thought doing so would aid the cause. Later, Erskine became minister of Old Greyfriars Church in Edinburgh and a leader of the evangelical party in the Church of Scotland.[29] The contacts between the Scottish revivalists and their American counterparts are the most impressive set of bilateral relations in the revival, since they did not rely on any single figure and involved some relatively obscure individuals.[30]

Just beyond this inner core was another group of promoters who, though less closely connected, nonetheless had a broad range of revival correspondents. These ministers, lay evangelists, financial backers, and printers were all in touch with Whitefield and at least one other leading revival minister, with several of their own local revival ministers, and sometimes with one another. Included in their number were London printers John Lewis and Samuel Mason, Boston printer Daniel Henchman, and lay promoters such as Thomas Noble of New York, Samuel Hazard of Philadelphia, and Ann Dutton of Huntingdonshire, England.[31] The

[28] The major manuscript, biographical, and periodical sources for this analysis are cited elsewhere in these notes. For a full discussion and citation, see Susan Durden, "Transatlantic Communications and Influence during the Great Awakening: A Comparative Study of British and American Revivalism, 1730–1760" (Ph.D. dissertation, Hull University, 1978).

[29] Erskine to Hall, July 15, 1743, MHS, Erskine Papers, 28–37; and Erskine to Cooper, January 8, 1743, MHS, Erskine Papers, 8–14.

[30] There is an extensive literature on the economic, cultural, and religious links between Scotland and America. See, for example, John Clive and Bernard Bailyn, "England's Cultural Provinces: Scotland and America," *William and Mary Quarterly*, 3d ser., 11 (1954): 200–13; Andrew Hook, *Scotland and America; A Study of Cultural Relations 1750–1835* (Glasgow, 1975); and *Scottish Historical Review*, 63 (1984). The latter volume is entirely devoted to essays on Scottish-American connections.

[31] Daniel Henchman's Ledgers and Wastbook are part of the Hancock Collection, Harvard Business School, Boston. Thomas Noble was a merchant, one-time High Constable of New York, and a Moravian sympathizer who spent over £100 on revival texts from Henchman. He also commissioned Benjamin Franklin to print revival sermons and Whitefield's journals. See George S. Eddy, *Account Books Kept by Benjamin Franklin, Ledger 'D'* (New York, 1929). Samuel Hazard had many business and religious contacts in Britain and the colonies and acted as a clearing house for transatlantic correspondence and packages. See Historical Society of Pennsylvania, Hazard Collection.

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only revivalist who does not fit easily into either the inner core or this second grouping is the Welsh preacher Howell Harris. He wrote prolifically to his associates in England and Wales but as far as is known had no American correspondents.[32] This lack of American contacts is surprising since Harris was more concerned than others were with maintaining the "catholic" or international spirit of the revival and, as one-time organizer at the London Tabernacle, was well placed to take part in a transatlantic network. Perhaps he failed to cultivate foreign correspondents because he felt his status as a mere "exhorter" was too lowly to warrant international attention. Even though he did not participate personally in the transatlantic communication network, his name was known abroad.[33]

THE NEW COMMUNITY CREATED BY INTERNATIONAL CORRESPONDENCE was, in part, a continuation of the seventeenth-century Puritan letter-writing community, but its spirit of evangelism marked a point of departure. Evidence suggests that revival correspondence was not only of personal significance to those involved but that it also served evangelical functions. Although letters between neighboring ministers were often of a practical nature—making arrangements for meetings and the exchange of pulpits, for example—those between distant and especially between international correspondents could be a means to convert the unconverted. In addition, because ministers discussed revival issues in their letters, their correspondence also helped to shape their attitudes to evangelism. These letters are a record of both the international workings of the revival and the issues that were of interest and concern to its leaders.

One of the primary and most straightforward purposes of letter writing was to describe the revival in one's own locality, and many of the revival letters were narrative and purely descriptive, particularly during the two years from 1740 to 1742 before the revival began to encounter opposition and internal disagreements. The same was true for correspondence within a region, but there was a heightened sense of excitement in writing to someone hundreds of miles away, and ministers were aware that news from afar could encourage the revival spirit in their own congregations: "In yours of Jan. 25 you acquainted me that what I wrote to Archibald Webster concerning the success of the Gospel in our American World, was most agreeable to you; and that it, with what was written by others, had refreshed many souls in my native country, Scotland, and filled their mouths with Praises."[34] Letters could be full of names of unknown ministers and places and details about individual converts, all written up in exuberant style: "I am now to inform you, that since my Last our exalted Saviour has been riding forth in his magnificence and Glory . . . , both Whites and Blacks, both Old and Young, both Prophane and Moral, awakened and made alive to God . . . at Taunton,

[32] The Trevecka Collection, University Library of Wales, Aberystwyth, includes about 3,000 of Harris's letters. See Roberts, *Trevecka Letters* and Tom Benyon, ed., *Howell Harris, Reformer and Soldier, 1714–1733*, Trevecka Records Series, no. 2 (Caernarvon, 1958).

[33] Tom Benyon, ed., *Howell Harris's Visits to London* (Aberystwyth, 1960), 43.

[34] Abercrombie (New England) to Leslie (Scotland), May 25, 1742, *Glasgow Weekly History*, no. 34, pp. 1–4.

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Bridgewater, Abington, York, Ipswich, Rowley, Cape Ann."[35] Despite the distance between correspondents and their probable ignorance of the local geography, the letters contained catalogues of new revival areas, and ministers often requested such detail from one another. They attached great importance to explicitness, both because it helped to establish the authenticity of the revival and, they believed, "it had more influence because it came from a Foreigner."[36] Through narrative detail, correspondents were able to encourage one another and develop a sense of unity in a known and shared world.

As part of their descriptions, ministers included information about their own conduct and leadership. The exchange of information can be seen as an early development of revival techniques, because the communication of various methods and approaches and discussion of their effects encouraged a process of convergence within evangelicalism. Presbyterian minister John Moorhead of Boston was one of the many who gave information about the methods he used in his parish. He encouraged parishioners to examine their own thoughts and feelings, listen to itinerant preachers, organize fasts, and heed the reading by heads of families of the "most pungent Discourses they could find that treated on the Nature of Conversion and the New Birth." Although he emphasized the educative role of societies for prayer and instruction, he was concerned that these should be properly organized by the minister.[37]

Ministers who exchanged views in this way did not always find it easy to decide about the limits of their own role or the efficacy of human effort in bringing about conversion, but in spite of their qualms, they were influenced by the apparent success of this or that particular practice. They occasionally discussed the controversial issue of revivalistic "means to grace." "We ought not only to praise God for everything that appears favourable to the interests of religion, and to pray earnestly for a general revival, but also use means that are proper in order to it," Edwards wrote to Erskine. The practice under scrutiny here was weekly sacraments, which Edwards considered "a proper means," and which Erskine said he had been striving to establish in the face of "bigotry and prejudice."[38] Other revival means discussed regularly by correspondents included the use of prayer societies, the practice of itinerant preaching, and coordinated prayer days—all of which were recognized as potentially threatening to a minister's control. Besides offering practical advice, these epistolary exchanges were of more general significance, since they influenced the writing of revival tracts, devotional materials, and theological treatises of the revival. The writers formulated their opinions within a transatlantic context and those opinions in turn established a number of views and patterns of response within that same transatlantic world. A key example of this process was the conversion experience.

[35] Prince, Sr., to Whitefield, December 6, 1741, *Glasgow Weekly History*, no. 15, pp. 1–3.
[36] *Christian Monthly History*, vol. 1, no. 4, p. 5.
[37] Moorhead to a Gent. in N. Britain, June 14, 1743, *Christian Monthly History*, vol. 1, no. 2, pp. 11–23.
[38] Edwards to Erskine, November 15, 1750 and July 5, 1750; Dwight, *President Edwards*, 1: 417 and 407–09.

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Although conversion was a personal experience, a matter between God and the individual, it was also of crucial interest to ministers. Perhaps because of the high regard in which New England was held by British revivalists, and certainly because of Edwards's writings on conversion, British ministers discussed features of the conversion experience with their American correspondents. In seeking to resurrect the centrality of individual conversion, revivalists drew on Puritan traditions, reprinting the works of seventeenth-century divines and recommending these to "seekers."[39] But the existence of a well-worked tradition did not obviate their own need to discuss contemporary conversions and their own particular experiences as the human promoters and arbiters of salvation and grace. Indeed, letters dealing with possible ways to distinguish a valid from a non-valid experience might be 5,000 words long and supported by specific instances and cases for discussion.[40] Ministers showed a desire to define and categorize the workings of grace, and, as their seventeenth-century counterparts had done, they provided models of the conversion experience that influenced the laity. A rare glimpse of this influence at work is afforded by Sarah Gill's diary entry for April 1742: "The next sabbath I heard dear Mr. Edwards," she wrote, "I trust I had the Presence of Christ with me, and *by the marks laid down* I concluded I had been drawn to Christ."[41] Ministers recommended to one another the practice of interviewing parishioners and questioning them closely on their spiritual life. John Moorhead explained how he made "each Person successively relate how far they had been acquainted with a work of conviction and Conversion upon their Souls."[42] This was also the practice at Cambuslang, as the 110 conversion accounts taken down by McCulloch show.[43] The many marginal notes and comments made by these ministers throughout the Cambuslang conversion accounts being prepared for publication reflected their concern to sort out proper from improper experiences. James Robe of Kilsyth was happy to share his own method with others: "I have kept a book wherein from Day to Day, I wrote down whatever was most material in the Exercise of the Distrest. This may appear an unsupportable Labour at first view, especially where the Number of the Distrest is so many. Yet I found it to be very easy. . . . An Index I kept, brought me soon to the part of the Book, where the Person's case was recorded. . . . I saw what Progress their Convictions had made, and knew where I was to begin with them." Robe published many of these cases in his Kilsyth

[39] For a discussion of the literature of the revival, including both the reprinting of Puritan literature and the publication of new revival materials, see Susan Durden, "Transatlantic Communications," chap. 5.

[40] See, for example, Erskine to Prince, Sr., July 17, 1742, MHS, Erskine Papers.

[41] Boston Public Library, MS Religious Journal of Sarah Gill. The emphasis is mine. For another instance of ministerial influence, see Michael J. Crawford, ed., "The Spiritual Travels of Nathan Cole," *William and Mary Quarterly*, 3d ser., 33 (1976): 98. For a discussion of Edwards's influence on the morphology of conversion, see Goen, *Great Awakening*, 25–32.

[42] *Christian Monthly History*, vol. 1, no. 4, p. 14.

[43] New College Library, Edinburgh, MS. W. 13. b. 212, "Examination of Persons under Scriptural Concern at Cambuslang during the Revival in 1741–42 by the Revd. William MacCulloch, Minister at Cambuslang, with Marginal Notes by Dr. Webster and Other Ministers." The two volumes are paginated separately and individuals identified by a two-letter system. References here are to pages.

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Narrative, which was reprinted in America and Holland and extracted in the Boston *Christian History*, where the editor recommended Robe's methods.[44]

Detecting a "defective" conversion was probably one of the most difficult and serious problems faced by a revival minister, but the heightened emotional tension so often inherent in a revival meeting could cause other problems. Were trances and dreams, for example, legitimized by Scripture? In New England, *Christian History* put across the view that "outcries" and excesses of expression were only to be expected but should not be taken as signifying conversion without other indicators. Aware that none of these experiences were new, the editor generally attempted to play down the emotionalism in the hope that people would thereby be discouraged from "indulging" themselves. As J. William T. Youngs has argued, many of these activities worried the very ministers who had encouraged the revival as a way to reunite and spiritualize the community, and who subsequently came to recognize that the same forces could impinge on their authority.[45] Troubled by these conflicting concerns, ministers could at least turn to a sympathetic community to voice their fears and obtain advice.

BECAUSE PRIVATE CORRESPONDENCE INTENSIFIED their emotional identification with one another and reinforced a set of beliefs and practices, revival leaders soon realized that its usefulness could be multiplied if the news and information related in the letters were shared more widely. One simple and obvious method was to pass a letter around among friends. Another was to make a copy and pass the letter on, or to read it aloud to a congregation or prayer group. But the most sophisticated technique evolved by evangelicals was to found newspapers and magazines whose main content was revival letters. The first and most natural step was to use the ready-made networks of friends and colleagues among whom letters could be circulated. New England ministers, for example, circulated letters at their regular association meetings, which were neighborhood affairs, while others added new forms to achieve the same effect. Scottish ministers began a special "correspondent meeting," which reflected their desire to be more organized in their evangelism.[46] Originally, few of the letters read aloud at these meetings would have been written for such a purpose, but once ministers established the practice of public readings,

[44] James Robe, *A Faithful Narrative of the Extraordinary Work of One Spirit of God at Kilsyth and other Congregations in the Neighbourhood* (Glasgow, 1742), reprinted in Gillies, *Historical Collections* (1754; reprint edn., 1981), 450. The *Narrative* was reprinted in *Christian History* (Boston, 1743), nos. 1–7 and went into three editions in Holland.

[45] J. William T. Youngs, Jr., *God's Messengers: Religious Leadership in Colonial New England 1700–1750* (Baltimore, 1976), 109–19. Robe and Edwards agreed that the people needed to be taught that "extraordinary joys and raptures" did not necessarily indicate a work of grace. Dwight, *President Edwards*, 1: 200–01.

[46] *Christian Monthly History* (Edinburgh), vol. 2, no. 8, p. 237. The English and Welsh Calvinist Methodists were organized through a monthly meeting of the whole society and weekly conferences of ministers and exhorters. In Cornwall, Anglican ministers held their own associations. See George C. B. Davies, *Early Cornish Evangelicals, 1735–1760: A Study of Walker of Truro and Others* (London, 1951). The development of ministers' associations in New England, and their role in the professionalization of the clergy has been well documented. See David D. Hall, *Faithful Shepherd: A History of the New England Ministry in the Seventeenth Century* (Chapel Hill, N.C., 1972); Selement, *Keepers of the Vineyard*; and Youngs, *God's Messengers*.

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it affected the style of the letters, making many of them more formal and impersonal. In England and Wales, where distances hindered meetings of like-minded ministers, workers at the London Tabernacle undertook a letter-transcribing service for revivalists outside London. Howell Harris provided this service for a time, as did John Syms, although they frequently complained that they lacked the work force to do the job thoroughly.[47] Instances of letter copying were to be found in Scotland and New England, too, but the practice was never as centralized or as systematic as it was in England and was likely to happen only if a letter were of exceptional interest. Daniel Wadsworth of Hartford, Connecticut, for example, recorded in his diary that he "saw a copy of a letter from Rowlands to Noble giving wonders wrought by his preaching" in Wales, and the same letter appears in the copybook of Ebenezer Parkman of Westborough, Massachusetts.[48] Even areas remote from the centers of communication could be brought in touch through the extension of local or internal networks. John Bonar of Torpichen wrote to McCulloch that he had heard of the revival in Cambuslang and other places in Britain and America and asked for "a particular account about this matter," which he received almost by return mail.[49] Henry Davidson of isolated Galashiels in Scotland cultivated fruitful exchanges with the Reverend Thomas Davidson of Braintree, Essex, England, and with the merchant William Hogg of Edinburgh, who in turn had contacts with the London Tabernacle and received American letters arriving in London and Edinburgh.[50] Similarly, Ebenezer Parkman conducted correspondence with Gilbert Tennant, Jonathan Edwards, and William Cooper, who were other colonials also involved in the transatlantic network, and with local ministers Andrew Eliot, John Webb, and Samuel Bliss.[51]

After circulating correspondence at their meetings and in private exchanges, ministers began to read letters, or at least some of the more stirring parts of them, to their congregations, in the hope that this experience would encourage people and communicate the idea of a special Providence from God. William McCulloch "frequently read to his hearers missives, attestations and journals which he had received from correspondents, giving an account of conversions which had taken place in different parts of the world, especially in New England."[52] One of his congregation, nineteen-year-old Elizabeth Jackson, included in her conversion account specific reference to the impact that McCulloch's readings had made on her: "Hearing a minister on a fast day, after a sermon, read some papers relating

[47] Roberts, *Selected Trevecka Letters*, 152.

[48] *Diary of Rev. Daniel Wadsworth* (Hartford, Conn., 1894), 55. Wadsworth had correspondence with Philip Doddridge, Benjamin Colman, and Jonathan Edwards and he subscribed to *Christian History* (Boston). MHS, Parkman MS, Copybook.

[49] *Edinburgh Christian Instructor*, September 1839, pp. 362–63, and 341n. "Among the Correspondents of Dr. Gillies, Mr. McCulloch and Rev. M'Laurin, we meet the names of Rev. James M'Lellan of Kirkowen, Thomas Forbe of Slains, James Smith of Kincardie, John Miller of Dunrossness, George Campbell of Botriphnie, W. Carlyle of Prestonpans, John Willison of Dundee and many more in Ross-shire."

[50] Henry Davidson, *Letters to Christian Friends* (Edinburgh, 1811), p. 43, *et passim.*

[51] Francis G. Walett, ed., "The Diary of Ebenezer Parkman," *Proceedings of the American Antiquarian Association*, 72 (April 1962): 31–233; and (October 1962): 329–481.

[52] John Sinclair, ed., *The Old Statistical Account* (Edinburgh, 1793), 267.

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to the success of the Gospel abroad, I was greatly affected at the thought that so many were getting good, and I was getting none." Another, Margaret Richardson, "was very glad to hear that there was such a work of conversion in those distant places [New England] and was very busy from time to time contriving methods how [she] might get there."[53] By the late 1740s, Jonathan Edwards was able to reflect with some satisfaction on his own part in spreading the news. Implicit in his recollection is a sense of the processes and stages by which information moved outward: first, "by taking great pains to communicate to others" and, then, equal pains "to extract from all letters" received. He went on to make the contents of this correspondence public by readings to his congregation "and also to the association of ministers . . . and occasionally to many others." This work did not exhaust his evangelical outreach, for he copied sections of letters and sent them to other parts of Massachusetts and to Connecticut with advice to the recipient that he too should "communicate it to other ministers and . . . to his people."[54] It is difficult to imagine how any individual could have done more without intervening directly.

Public readings of foreign and domestic news known as Letter Days became institutionalized as a regular part of the society calendar in England and Wales, through the organization of the Calvinist Methodist Tabernacles. Limitation of the practice to these two countries may reflect the lower levels of literacy and stronger oral traditions in England and Wales as compared to New England and lowland Scotland, especially among those involved in the revival.[55] The English revivalists were also directly affected by the Church of the Brethren (the Moravians), a German pietist community that had been in England since 1735 under its leader Count Nikolaus Ludwig von Zinzendorf. The Moravian practice of *Gemeintag*, or mass meeting for the reading of missionary reports, impressed the new revival societies in England and Wales, which were particularly susceptible to Moravian influences. *Gemeintag* had been central to Moravian community life since 1728, and although it included ordinations, marriages, and prayer, the main feature was "the communication of the latest accounts from the churches and missions from all parts."[56]

The Moravians conducted their Letter Days wherever they went including the Fetter Lane Society in London, whose meetings Whitefield, the Wesleys, and other members of the Oxford Club attended. As early as 1739, English evangelicals had adopted the practice of Letter Days.[57] After separation from the Moravians in 1741, Whitefield and the Wesleys started their own Letter Day at the society in the Foundry in London, although they and their followers continued to attend the Moravian Letter Day, too. Howell Harris recorded in his diary that he "went to

[53] "Examination of Persons under Spiritual Concern at Cambuslang," 1: 103, 2: 333.

[54] Edwards to McCulloch, May 23, 1749, Dwight, *President Edwards*, 1: 276–77.

[55] See T. C. Smout, "Born Again at Cambuslang: New Evidence on Popular Religion and Literacy in Eighteenth Century Scotland," *Past and Present*, 97 (1982): 114–27.

[56] John M. Levering, *A History of Bethlehem, Pennsylvania 1741–1892* (Bethlehem, Pa., 1903), 67; Walsh, "Cambridge Methodists," 263.

[57] From Bristol, Wesley wrote to London publisher and Methodist, James Sutton, "I wish you would constantly send me extracts of all your foreign letters, to be read out on Intercession Day." John Telford, ed., *Letters of John Wesley*, 8 vols. (London, 1960), 1: 314.

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hear the letters read in the Foundry . . . then again to Fetter Lane to hear the Moravian letters." And in a letter to Daniel Rowlands, which reflected the early pluralism of the revival, Harris reported that "yesterday we Whitefield's Methodists had our monthly Day for reading letters about the Progress of the Gospel elsewhere—such accounts as we had from America and Scotland your ears never heard of. . . . Yesterday was the Letter Day with the Moravians where was glorious accounts from Pennsylvania."[58] The Calvinist Methodists established regular Letter Days as early as 1741, meeting in London on the second Monday of the month and in Bristol on the first Monday in the month.[59] These monthly readings continued for at least seven years as an important feature of the Whitefieldian movement.

A Letter Day assembly usually began with an exhortation followed by a reading of letters, each of which might be concluded by communal singing of a specially written verse. Another exhortation and prayer rounded off the service. The meeting lasted several hours and was an occasion of great excitement, "the Tabernacle being filled in all its Parts for Peoples sitting and many standing on the outsides of the seats."[60] Two sorts of letters were sent to the Tabernacle, one a personal letter donated by its recipient, the other a letter specially written to be read aloud and usually referred to as an "account" by the preachers who sent them in. Like the Epistles of Paul and the letters of George Fox to the Quakers, the second type of letter was consciously addressed to a large group of listeners and the style and content were adapted to that forum. Exhorters and superintendents of the societies were urged to send in a full account of their preaching, the persecutions they experienced, the new converts made, and new societies formed.

Letter Days, confined as they were to the centers of communication even in England, could not by themselves solve the problem of limited access to the news. From the outset, revivalists asked themselves how they could best multiply the effects they were achieving beyond their own immediate circles. The London printer John Lewis supplied the answer when, in September 1740, he began editing and printing a weekly revival newspaper.[61] His concern was with "the

[58] Harris's diary, October 4, 1742, Benyon, *Reformer and Soldier*, 26; and Harris to Rowlands, September 14, 1742, Roberts, *Selected Trevecka Letters*, 46.

[59] Luke Tyerman, *Life of the Rev. George Whitefield*, 2 vols. (London, 1876–77), 1: 542. Whitefield wrote to the Welsh evangelists, December 28, 1741: "I am about to settle a monthly meeting in Bristol and London where correspondents' letters are to be read." Also see Welch, *Two Calvinistic Methodist Chapels*, 19.

[60] *An Account of the Most Remarkable Particulars Relating to the Present Progress of the Gospel* (London, 1742), vol. 2, no. 2, p. 76.

[61] The sequence of publication was as follows: John Lewis, ed., *Christian's Amusement containing Letters Concerning the Progress of the Gospel both at Home and Abroad etc. together with an Account of the Waldenses and Albigenses . . .* (London, 1740), printed by John Lewis, September 1740–March 1741, 4 pp., price 1d. (hereafter, *CA*). Then: *Weekly History: Or, An Account of the Most Remarkable Particulars Relating to the Present Progress of the Gospel. By the Encouragement of the Rev. Mr. Whitefield* (London, 1741), vol. 1, printed by John Lewis, April 11, 1741–November 13, 1742, 4 pp., price 1d. (hereafter, *WH*). Then in Autumn 1742: *An Account of the Most Remarkable Particulars Relating to the Present Progress of the Gospel* (London, 1742), vols. 2, 3, and 4, printed by John Lewis, 84 pp., price 1/2 d. per week (hereafter, *Account*). Then in Autumn 1743, *Christian History or General Account of the Progress of the Gospel in England, Wales, Scotland and America as far as the Rev. Mr. Whitefield, his Fellow Labourers and Assistants are concerned* (London, 1743), vols. 5, 6, and 7, printed by John Lewis every seven weeks. Each volume was divided into four numbers of 84 pp. (hereafter, *CH*). Finally: *Christian History or General Account . . . of the Gospel*

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Progress of the Gospel both at Home and Abroad," and, from the first edition, he used letters as the main substance of the paper. Whitefield himself had seen the value of newspaper publicity, and, before Lewis's venture, had attempted to use the secular press to promote the revival. But he encountered too many obstacles to the success of this project, including a lack of interest by secular editors and difficulty in controlling his copy. He found an independent paper to be a more useful instrument. By April 1741, Whitefield had officially adopted Lewis's paper for publishing up-to-date revival news. From 1741 onward, the London paper became increasingly closely linked with Letter Day, eventually publishing little besides the letters read at the Tabernacle. Copyists transcribed letters received at the Tabernacle into books so that the leaders could make selections for publication and for public readings. Moreover, Lewis found imitators in Scotland and New England. William McCulloch of Glasgow, James Robe in Edinburgh, and Thomas Prince, Jr., of Boston took up Lewis's idea and edited revival papers or magazines. All of these ministers, as active members of the communications network, were well placed to take on this new role.[62] Even so, they relied heavily on one another to make a success of their papers. McCulloch drew on the *London Weekly History* for his *Glasgow Weekly History*, concentrating on reprinting letters to the near exclusion of other forms of literature. In his *Christian History*, Thomas Prince, Jr., aimed to print "extracts of written letters both from England, Scotland, New York, New Jersey, South Carolina and Georgia of a religious Nature as they shall be sent hither from creditable Persons and communicated to us." James Robe, who put together the *Christian Monthly History*, was aware that "a very extensive correspondence must be established for the carrying on a Design of this sort to purpose." Robe and Prince solicited letters for publication, but all the editors received unsolicited correspondence, including some from lay people. Once the editors had made contact with one another and exchanged complete sets of their own papers, coverage became as complete as was possible.[63]

Only this inexpensive and full reprinting of letters overcame the limitations of other methods of dissemination adopted in the early to mid-eighteenth century. It gave each English-speaking revival country its own newspaper or magazine that, for a few pence, brought the reader current with all the latest revival news. Individuals could read the newspapers privately or a minister could use them in

. . . (London, 1748), 1 volume printed by John Lewis containing letters June 1747–June 1748, 283 pp. (hereafter, *CH 1748*). The only complete collection is in the National Library of Wales, Aberystwyth. There is a discussion of these papers in Susan Durden, "A Study of the First Evangelical Magazines," *Journal of Ecclesiastical History*, 27 (1976): 255–75.

[62] William McCulloch, ed., *Glasgow-Weekly-History Relating to the Late Progress of the Gospel at Home and Abroad: Being a Collection of Letters partly reprinted from the London-Weekly-History* . . . (Glasgow, 1742), December 1741–December 1742, 8 pp., price 1d., bound with an index (hereafter, *GWH*); James Robe, ed., *Christian Monthly History or an Account of the Revival and Progress of Religion Abroad and at Home*, 2 vols. (Edinburgh, 1743), November 1743–January 1746 (hereafter, *CMH*); Thomas Prince, Jr., *Christian History, Containing Accounts of the Revival and Propagation of Religion in Great Britain and America . . . 1743 and 1744*, 2 vols. (Boston, Mass., 1743), March 5, 1743–February 23, 1745, 8 pp. (hereafter, *CH Boston*). See Durden, "First Evangelical Magazines."

[63] Prince, Jr., to McCulloch, December 13, 1743, New York Public Library MSS letters, J. H. Benton sale, American Art Association 1920 catalogue, 3d session, March 13.

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many different ways. In the Ross-shire village of Nigg, for example, John Balfour wrote to James Robe: "I procured most of your Narratives and Letters; and this Post I commission for an entire set of them, and for Copies of the *Christian Monthly History* . . . and have recommended to some others to procure them and will to more. . . . The Design is very laudable and has already been of great Use. It is a choice Means to promote the Communion of Saints upon Earth."[64] Three thousand miles away from Nigg, Nicholas Gilman of Durham, New Hampshire, read to his parishioners from the *Glasgow Weekly History* and lent copies out as part of his parish library scheme. From Somerset, England, Risdon Darracott, a Presbyterian minister, wrote thanking Lewis for his good work, telling him that he had "distributed the Weekly Papers among my People and they are much affected with them." John Harrison, Nonconformist minister in Braintree, Essex, thought the London papers particularly useful for keeping the young people out of temptation's way, which he did for nearly three hours at a time by reading "the substance of near 30 of Mr. Lewis's Papers."[65] All the papers were miscellanies of news from Scotland, England, Wales, the American colonies, and Holland, in the form of letters, narratives, and extracts from printed works. Each paper reflected the views and abilities of its editor. The Boston paper, for example, which came out weekly for two years, was purposefully organized by Prince to "reaffirm the special covenant meaning of New England's past, by associating the revival with Puritan traditions," but, even so, almost one-third of the paper contained news and articles from Scotland, with Prince drawing attention to the similarities between the two areas.[66]

Although evangelical papers and magazines were the most innovative and effective method for spreading the word, more traditional methods continued alongside them. Whitefield's correspondence, for example, found its way into print most commonly through his *Journals*, and he occasionally also had it printed on the back of sermons and pamphlets or used it to break up some longer and weighty piece of prose. Three American letters to Whitefield were bound between his own *Letter to New Brunswick* and *Letter in Answer to the Presbytery of Newcastle*. Whitefield and other revival leaders regarded these letters, and other personal correspondence, as general revival property that could be published for the edification of the community at large.[67] By combining their private networks for the dissemination of news and literature with their outlets for more general publication, revival leaders had an effective vehicle for launching specific projects or for conducting campaigns on an international level. In this way, they adopted

[64] *CMH*, vol. 1, no. 4.

[65] Nicholas Gilman MS Diary, New Hampshire Historical Society. See, for example, the entry for October 19, 1742. Darracott's letter was printed in *WH*, no. 46; Harrison's in *WH*, no. 52 and reprinted in *GWH*, no. 18, pp. 1–4.

[66] Jan E. Van de Wetering, "The Christian History of the Great Awakening," *Journal of Presbyterian History*, 44 (1966): 124.

[67] See, for example, *Copy of Three Letters, the first written by Dr. John Nicol at New York, to Mr. William Wardrobe, Surgeon in the Grass-market of Edinburgh; the second by a dissenting minister in England to a Gentleman in Scotland; the third from a minister at Boston to his friend at Glasgow, Giving an Account of the Progress and Success of the Gospel in Foreign Parts* (Edinburgh, 1740). For another example, see Dwight, *President Edwards*, 1: 407.

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and supported Whitefield's Georgia Orphan House, donated money to missionaries, and helped to establish the Presbyterian College of New Jersey.[68] Above all, they initiated and sustained the United Concert for Prayer, a parish-based international movement that became another of the major legacies bequeathed by the mid-eighteenth-century revivals to evanglical protestantism.

THE SUCCESSFUL ESTABLISHMENT OF COORDINATED PRAYER DAYS, organized as the United Concert for Prayer, is solid evidence for the effectiveness of the communication networks. Initially, revivalists simply adopted the Puritan tradition of days of prayer, and, by 1741, the practice of setting aside a named day for prayer and fasting for the general revival of religion was common among the various religious societies and meetings in Britain and America. At this date, each group acted independently of the others. Whitefield, for example, made his society's day the sixteenth of the month, and many Calvinist Methodist societies in England and Wales kept the same day. In Dundee, in March 1742, Church evangelicals "agreed to observe Thursday next for Thanksgiving to the Lord in all our prayer societies and others are invited to join in Praising the Lord."[69] Jonathan Edwards, too, recommended in *Some Thoughts Concerning the Present Revival,* published in 1742, that a shared day of prayer and fasting form the basis of a covenant between God and the converted. His *Thoughts* were widely read among Scottish revival activists and may have encouraged their own interest in shared prayer.[70] By 1743, the practice was well organized in Lowland Scotland: "There was a Proposal from the Praying Societies at Edinburgh transmitted in a short printed Memorial to us and other Places to set apart Friday the 18th now past for Thanksgiving . . . and Prayer. . . . There was a serious and apparent concern among the People."[71] In the space of two years, the prayer day had become increasingly organized. By 1744, these Scottish ministers were ready to take the concept a stage further. Their proposal in October for joint action with their English and American counterparts in a United Concert for Prayer created something new out of the old tradition. Societies and individuals would commit themselves to weekly and quarterly prayer times in which prayer would be offered for a universal revival of religion. The proposers drew societies in, partly by making use of the well-developed personal correspondence network and partly through the Edinburgh *Christian Monthly*

[68] Collections were continually made for the Orphanage in Britain. See Welch, *Two C M Chapels,* xv, 8, 13. On missionary work, it is interesting to note that correspondence across the Atlantic continued through the 1750s and 1760s, as for example between Eleazor Wheelock, founder of an Indian Charity School at Dartmouth, New Hampshire, and his British patrons. Leon B. Richardson, *An Indian Preacher in England* (Hanover, N.H., 1933). William McCulloch himself spent £227 on missionary work; Fawcett, *Cambuslang Revival,* 216. British revivalists were also warm supporters of New Jersey College and the correspondence across the Atlantic about its foundation survives in the Burr MSS, Princeton University Library. Rev. Samuel Davies went on a fund-raising trip to Britain on behalf of the College, 1753–55. See George W. Pilcher, *The Rev. Samuel Davies Abroad: The Diary of a Journey to England and Scotland, 1753–55* (London, 1955).

[69] *GWH,* no. 26, p. 7.

[70] Fawcett, *Cambuslang Revival,* 225.

[71] *Account,* vol. 3, no. 1, p. 18.

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History, which devoted a complete issue to the Concert.[72] The universal evangelism and catholic spirit of the Concert idea recommended it to the many shades of theological opinion within the revival movement,[73] and, when the two-year pledge period was over, a fresh Scottish proposal to continue for a further seven years was accepted. The new proposal, made in August 1746, took the form of a printed Memorial that could be circulated within the network. Edwards, as the main American supporter of the Concert, received 500 copies for distribution, which he seems to have redistributed using the "tree" method: "I have very lately received a Pacquet from Scotland," he wrote to Joseph Bellamy, "with several copies of a Memorial, for the continuing and propagating of an Agreement for joint Prayer, for the general Revival of religion, three of which I send to you, desiring you to dispose of two of 'em where they will be most serviceable."[74] When originally proposing the Concert, revivalists in Scotland had been hesitant about advertising it too publicly, possibly fearing that it might be regarded by some as "superstitious" and by others, especially employers, as "interfering with other duties."[75] By 1746, they were emboldened (perhaps because none of their fears were realized) to print and circulate the Memorial on the grounds that "notwithstanding of what may be done by private letters it is humbly expected that a Memorial . . . may reach where they will not."[76]

It is not easy to assess the number of societies and individuals associated with the Concert. Robe reported in 1749 that letters from New England told of the "great progress this Concert has made in these provinces. Many ministers, private Christians, yea congregations and churches, have entered into it and continue to enter."[77] But he gives no names or numbers. Certainly, the Concert received an additional boost from the publication by Edwards in 1747 of his *Humble Attempt to Promote Explicit Agreement and Visible Union of God's People in Extraordinary Prayers*, which gave the genesis and the history of the Concert and included the entire text of the 1746 Scottish Memorial. As late as 1754, revivalists maintained sufficient interest to ask for a second seven-year extension of the arrangement.

Evangelicals of the 1740s were eager to prove the existence of good Scriptual and Reformation precedents for the coordination of prayer, and they cited chapter and verse to support their case.[78] But their very strenuousness indicates an awareness that they were open to criticism for innovation. With benefit of hindsight, we can see that they had moved in the direction of a more instrumental approach to revivalism. Because it was planned on a large scale and was well organized and purposefully instrumental, the Concert, like the magazines, marked

[72] *CMH*, vol. 2, no. 1.
[73] John Wesley accepted an invitation to join, and himself suggested to Erskine that Edwards and Gilbert Tennent would probably agree to take part; Telford, *Letters of John Wesley*, 2: 33.
[74] Frank B. Dexter, ed., *Manuscripts of Jonathan Edwards* (Cambridge, Mass., 1901), January 15, 1746.
[75] Gillies, *Historical Collections* (1754; reprint edn., 1981), 462–64.
[76] Jonathan Edwards, *An Humble Attempt to Promote Explicit Agreement and Visible Union of God's People in Extraordinary Prayer for the Revival of Religion and the Advancement of Christ's Kingdom on Earth* (Boston, 1747), 23.
[77] Gillies, *Historical Collections*, 464.
[78] *Ibid.*, 462–64.

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a shift in position by evangelicals. A number of them had constituted themselves into a group, with George Whitefield at its center, and created an evangelical community that consciously, if gradually, transformed revival practices.

The revival activities described here provided a link between eighteenth and nineteenth-century revivalism. The Concert for Prayer, for example, was resuscitated in the 1780s by a group of Northamptonshire English Baptists. When this group, which included Pastor Andrew Fuller, received a copy of Jonathan Edwards's *Humble Attempt*, in a parcel of books sent by John Erskine, they agreed to try to promote a revival of religion through a "prayer call."[79] In its turn, the prayer call was claimed by William Carey as the spiritual inspiration behind his founding of the Baptist Missionary Society (1792). These late eighteenth-century evangelicals were undoubtedly less hesitant about using these means for promoting revival than their original proponents had been, and the confidence of later revivalists came in part from knowledge of precedent and example. The Concert came to them with a solid evangelical pedigree; they did not have to pioneer this form.

The mid-eighteenth-century revivalists had created the first evangelical magazines. Even if the appearance of these magazines accompanied a general expansion of periodical literature and of a magazine-reading public, the publication of evangelical magazines at this early date was not inevitable. Given the sheer effort required of amateurs to collect copy, print, distribute, and finance such a publication, it is hardly surprising that the magazines died when the revival impulse flagged. The absence of institutional support systems led in the 1750s to a lull in the publication of the magazines, despite their usefulness to evangelicals. By the last twenty years of the century, however, the magazines revived and became an integral and vital element in Christian outreach, so that in the nineteenth century, evangelicals often used the periodical press to promote and encourage revivals in ways reminiscent of the *Christian's Amusement* and its successors. These later publications of course reflected the growth of denominationalism and the concomitant decline of ecumenical protestant evangelicalism. Historians of nineteenth-century revivalism have not neglected these magazines as source material, or failed to see in them the mark of a more instrumental revivalism.

THE HISTORY OF CONNECTION, interconnection, and direct assistance between evangelicals in different countries and across generations is understandably recounted with triumph by evangelical historians, even though they have shown less interest in the human agencies involved. Other historians of the same events have been well aware of this evangelical tradition, relying as they have on the materials collected by men such as Prince or Gillies, who recorded the details of chronology and location but did not regard them as significant. A familiar portrait of revival history has emerged from the evangelical tradition, painted with broad,

[79] These connections are traced in Fawcett, *Cambuslang Revival*, 223–33; Ernest A. Payne, *Prayer Call of 1784* (London, 1941), and Watts, *Dissenters*, 456–61.

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bold strokes on a large canvas. In their turn, social historians have sought to get beyond these generalities to the particular nature of a revival in a specific place and at a specific period. By setting the religious behavior of mid-eighteenth-century men and women in the context of local social and political structures, they have rightly enmeshed the revival in community affairs. In their work, human activities and human emotions have replaced the hand of God as the focus of interest.

The evidence presented here shows that it is not necessary or helpful for historians to focus solely on local events. Revivals did indeed take place within particular communities, and we need to understand the complex relationships between revivalism and local religious and social groupings. But the international dimension of evangelicalism in the transatlantic world of the 1740s also needs to be recognized and understood. This dimension is as capable of explanation as the local context, and the human agencies are just as visible. The broad dissemination of news did not necessarily cause revivals, but the exciting knowledge that clergy and lay people acquired about events elsewhere shaped individual behavior and individual understanding of what the Lord had "upon the Wheel."

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Insects and the European Mortality Decline

JAMES C. RILEY

CRUDE DEATH RATES IN EUROPE BEGAN TO FALL AFTER 1670, with variations from place to place in the beginning date of this decline. By the mid-twentieth century, death rates had declined from between 30 and 40 per 1000 each year to around 10 per 1000.[1] The decline, one of the most important features of European experience in the modern era, occurred in two phases. The two are separated by a mortality plateau, which characterized much of Europe during rapid urban growth in the middle decades of the nineteenth century. Growing cities, known historically as sites of excessive fatalities, interrupted the decline until the latter half of the nineteenth century, when public health reforms and an improved standard of living reestablished it.

Each phase of the mortality decline presents its own problems of explanation and interpretation. The first accounts for approximately half the overall decline in death rates. By the 1820s, when urban growth intervened, crude death rates in western Europe had fallen in many areas to around 20 per 1000 per annum. This

I am grateful to Frank N. Young and the referees consulted by this journal for their advice about earlier versions of this essay.

[1] E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541–1871* (Cambridge, Mass., 1981); R. E. Jones, "Further Evidence on the Decline in Infant Mortality in Pre-Industrial England: North Shropshire, 1561–1810," *Population Studies* (hereafter, *PS*), 34 (1980): 239–50, esp. 240 and 247; Jacques Dupâquier, *La population française aux XVII^e et XVIII^e siècles* (Paris, 1979); Yves Blayo, "La mortalité en France de 1740 à 1829," *Population*, 30, Special Number (1975): 123–42, esp. 129; Jacques Houdaille, "La mortalité des enfants dans la France rurale de 1690 à 1779," *Population*, 39 (1984): 77–106; André Armengaud, *La famille et l'enfant en France et en Angleterre du XVI^e au XVIII^e siècle: Aspects démographiques* (Paris, 1975), 77–80; Arthur E. Imhof, *Aspekte der Bevölkerungsentwicklung in den nordischen Ländern, 1720–1750*, 2 vols. (Bern, 1976); Erland Hofsten and Hans Lundström, *Swedish Population History: Main Trends from 1750 to 1970* (Stockholm, 1976); Anders Norberg, Hans Norman, and Sune Åkerman, "Regional and Local Variations of Mortality in Sweden, 1750–1900," *Scandinavian Population Studies* (hereafter, *SPS*), 5 (1979): 55–73; Otto Andersen, "The Development of Danish Mortality, 1735–1850," *SPS*, 5 (1979): 9–21; Oiva Turpeinen, "Fertility and Mortality in Finland since 1750," *PS*, 33 (1979): 101–14; Kari Pitkanen, "The Changing Features of Mortality in Finland in the 18th and 19th Centuries," *SPS*, 5 (1979): 74–82; Michael Drake, *Population and Society in Norway, 1735–1865* (Cambridge, 1969); W. Robert Lee, "The Mechanism of Mortality Change in Germany, 1750–1850," *Medizinhistorisches Journal*, 15 (1980): 244–68; Arthur E. Imhof, "From the Old Mortality Pattern to the New: Implications of a Radical Change from the Sixteenth to the Twentieth Century," *Bulletin of the History of Medicine* (hereafter, *BHM*), 59 (1985): 1–29; J. A. Faber, *et al.*, "Population Changes and Economic Developments in the Netherlands: A Historical Survey," *A.A.G. Bijdragen*, 12 (1965): 47–110; and W. R. Lee, ed., *European Demography and Economic Growth* (New York, 1979). As the literature cited here and below reveals, available evidence on this early period is most plentiful for western and northern Europe, where the mortality decline is believed to have begun and where useful public health measures were first introduced.

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half of the overall decline occurred in a period before medical or chemical therapies are believed to have been generally efficacious, before introduction of public health measures associated with the late nineteenth century (especially water purification), and during an era of inflation in the price of basic foodstuffs.

To find reasons for the first phase of the decline, it is necessary to consider the age structure of mortality, the diseases especially important in seventeenth and eighteenth-century mortality, the means by which those diseases were transmitted, the introduction and timing of measures that may have impeded their transmission, and the spatial distribution of the mortality decline, which appears to have begun in western Europe and to have moved slowly eastward. Many causes of death common in the pre-decline era, including diseases with high and moderate case-fatality rates, are associated with filth or insects, specifically, mosquitoes, flies, lice, fleas, and probably cockroaches. All of these insects are small and, whether operating as mechanical or biological vectors in transmitting disease, their contacts with people are largely haphazard. All are known to be capable, in appropriate conditions of filth, of carrying large numbers of pathogenic microorganisms or quantities of pathogenic matter. The leading issue in surveying the insect role in disease transmission is, however, insect numbers. Epidemics of diseases transmitted by arthropods are known to be likely when insect populations are dense and unlikely when these populations are sparse. The probable density of insect populations in the seventeenth century and the introduction of measures that reduced this density deserve attention in any assessment of the mortality decline.

THE FIRST PHASE OF THE MORTALITY DECLINE HAS SEEMED ESPECIALLY RESISTANT to explanation. Attempts to account for it can be divided into two groups. The first of these, which has been dominant in recent secondary writing, seeks an explanation in the changing consumption of food, under a general notion that levels of nutrition influence both the likelihood that disease will be contracted and the probability that a sick person will die.[2] Drawing on experience with severe malnutrition in underdeveloped twentieth-century economies, proponents of this explanation have examined relationships between harvest crises and mortality, the long-run trend of temperature, and the aggregate output and distribution of foodstuffs. These efforts have produced inconclusive results. Twentieth-century underdeveloped economies that have suffered severe malnourishment prove to be a weak basis for reconstructing early modern European experience, because several of the economies of early modern Europe are known to have had income levels superior to some underdeveloped twentieth-century economies, because important differences in the breadth, frequency, and impact of famine conditions prevent a useful comparison, and because eighteenth-century experience points toward undernourishment problems generally less serious than those of severe malnutrition. Severe malnutrition is known to weaken the immune response, but

[2] Thomas McKeown, *The Modern Rise of Population* (London, 1976), and "Fertility, Mortality and Causes of Death: An Examination of Issues Related to the Modern Rise of Population," *PS*, 32 (1978): 535–42.

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undernourishment has more ambiguous health effects. Undernourishment does not appear to influence the likelihood of contracting disease, and it may militate against the multiplication of pathogenic microorganisms, which themselves require nourishment.[3] Twentieth-century cases of severe malnutrition among people ordinarily receiving adequate nourishment, such as in the Netherlands at the end of World War II, indicate that, in these circumstances, the physical and pathological effects of famine are short lived in most individuals.[4] Studies of early modern harvest crises show that mortality peaked during and after harvest failures, but, in general, these crises became rare before or in the earliest stages of the mortality decline. The shift from frequent to infrequent harvest crises, which most historians associate with the late sixteenth century in Britain and the seventeenth century in western and central Europe, may therefore help explain the initiation of mortality decline, but it cannot explain its continuation.[5]

Studies of climate and temperature trends have not provided reasons to expect rising levels of caloric intake. This research shows little long-run variation in mean annual or growing season temperatures.[6] But the most compelling reason for skepticism about the theory that mortality rates declined because consumption levels improved is provided by price evidence. The inflation of food prices from about 1730 to 1815 appears to have occurred throughout the western half of Europe and signals a more rapid increase in the price of foodstuffs than in incomes.[7] Improvements in nutrition would be most likely to have affected

[3] J. G. Hirsch, "Host Resistance to Infectious Diseases," in René J. Dubos and James G. Hirsch, eds., *Bacterial and Mycotic Infections of Man* (4th ed.; Philadelphia, 1965), 176–77. A collaborative effort to distribute major European diseases according to the influence of nutrition on their outcome appeared in a conference volume of the *Journal of Interdisciplinary History* (hereafter, *JIH*): "The Relationship of Nutrition, Disease, and Social Conditions: A Graphical Presentation," *JIH*, 14 (1983): 506, refining an article by Andrew Appleby, "Nutrition and Disease: The Case of London, 1550–1750," *JIH*, 6 (1975): 1–22. According to this report, the diseases of leading importance in seventeenth and eighteenth-century mortality are divided more or less evenly in three categories: a definite influence, an equivocal or variable influence, and a minimal influence. The allegedly definite association between cholera and tuberculosis, on the one hand, and nutrition, on the other, seems to weaken the nutrition argument. Both diseases are believed to have peaked in the nineteenth century. If nutrition improved in the way asserted by McKeown for England—steadily from early in the eighteenth century (*Modern Rise*, 147)—then mortality rates from at least tuberculosis should have declined.

[4] G. C. E. Burger, J. C. Drummond, and H. R. Sandstead, eds., *Malnutrition and Starvation in Western Netherlands, September 1944–July 1945* (The Hague, 1948), esp. 165–67, showing both that the famine produced severe hunger-edema, more tuberculosis, and higher infant mortality, and that these effects were quickly compensated for by renewal of consumption.

[5] Jean Meuvret, "Les crises de subsistances et la démographie de la France d'ancien régime," *Population*, 1 (1946): 643–50; François Lebrun, "Les crises démographiques en France aux XVII[e] et XVIII[e] siècles," *Annales: Economies, Sociétés, Civilisations* (hereafter, *AESC*), 35 (1980): 205–34; and Claude Bruneel, *La mortalité dans les campagnes: le duché de Brabant aux XVII[e] et XVIII[e] siècles*, 2 vols. (Louvain, 1977), 1: 603. Also see Tommy Bengtsson and Rolf Ohlsson, "Age-Specific Mortality and Short-Term Changes in the Standard of Living: Sweden, 1751–1800" (unpublished), 13–14; Oiva Turpeinen, "Les causes des fluctuations annuelles du taux de mortalité finlandois entre 1750 et 1806," *Annales de démographie historique* (1980): 287–96; Lee, "Mechanism of Mortality Change," 266; and Andrew B. Appleby, *Famine in Tudor and Stuart England* (Stanford, Calif., 1978), esp. 109–54.

[6] H. H. Lamb, *Climate: Present, Past and Future*, 2 vols. (London, 1972–77), 2: 481.

[7] On incomes, see Paul Bairoch, "Europe's Gross National Product: 1800–1975," *Journal of European Economic History*, 5 (1976): 273–340. Among the price histories, see especially N. W. Posthumus, *Inquiry into the History of Prices in Holland*, 2 vols. (Leiden, 1946–64), 1: xcv, *et passim*; Ernest Labrousse, "Les 'bon prix' agricoles du XVIII[e] siècle," in Fernand Braudel and Ernest Labrousse, eds., *Histoire économique et sociale de la France*, 4 vols. (Paris, 1970), 2: 387; and Lennart Jörberg, *A History of Prices in Sweden, 1732–1914*, 2 vols. (Lund, 1972).

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mortality rates among the poorest segments of the population, but these segments were least likely to have been able to buy better nutrition in an era of rising prices. Even in England, for which scholars have made the strongest case for the effects of agricultural improvements, the growth rate of wheat yields was probably not as great as the growth rate of the population.[8]

Dietary changes offer a more promising line of inquiry. Considering infant and general mortality in London, Mary K. Matossian suggested that a shift in the diet of the poor from rye (prone to fungus infection) to potatoes may explain part of the late eighteenth and early nineteenth-century decline in infant mortality in London, a decline that she associated especially with fewer deaths from convulsions.[9] These possibilities can be explored by considering likely effects from contaminants associated with foods widely eaten, by looking at what price data suggest about dietary patterns, and by further studies on what changing adult heights and weights suggest about nutrition in early life.[10]

A second group of explanations, in bad repute recently among scholars, suggests that the mortality decline may be associated with medical or public health remedies. Until the publication in 1955 of a persuasive review of eighteenth-century therapies by Thomas McKeown and R. G. Brown, historians were usually willing to accept the claims of early modern physicians who asserted that their own treatments were effective.[11] Neither the therapies in general use in that period nor those introduced then seem likely, however, to explain a mortality decline. Most physicians, including as prominent a figure in clinical medicine as Hermann Boerhaave, continued to rely on the standard treatments of humoral therapy, including bleeding and purging, and therefore on therapies unlikely to have been effective. The leading innovation, smallpox inoculation, is also an unsatisfactory explanation for the decline.[12] Outside Britain, variolation was not widely practiced until the late eighteenth century, and, as for its effects inside Britain, some authorities remain skeptical about the capacity of one preventive treatment, specifically, this crude treatment, to influence mortality rates in a large way. Vaccination, which became common in the early decades of the nineteenth

[8] Mark Overton, "Agricultural Productivity in Eighteenth-Century England: Some Further Speculations," *Economic History Review*, 37 (1984): 244–51. Improvements in transportation and communication appear to have kept pace with the rising demand for foods but not to have made a major contribution to distributing foods more efficiently. Market integration, which signals the effects of improvements in transportation and communication, seems to have been more robust in the seventeenth than in the eighteenth century. Louise Tilly, "The Food Riot as a Form of Political Conflict in France," *JIH*, 2 (1971): 23–57, esp. 45.

[9] Mary Kilbourne Matossian, "Death in London, 1750–1909," *JIH*, 16 (1985): 183–97.

[10] John Komlos, "Stature and Nutrition in the Habsburg Monarchy: The Standard of Living and Economic Development in the Eighteenth Century," *AHR*, 90 (1985): 1149–61.

[11] T. McKeown and R. G. Brown, "Medical Evidence Related to English Population Changes in the Eighteenth Century," *PS*, 9 (1955): 119–41.

[12] On the practice in England, see Peter Razzell, *The Conquest of Smallpox: The Impact of Inoculation on Smallpox Mortality in Eighteenth Century Britain* (London, 1977). Among therapeutic innovations, quinine was introduced in the seventeenth century and given in appropriate dosages in the late eighteenth century, but it is unlikely that the symptomatic relief it offered influenced mortality rates. Scurvy was likewise treated effectively in the late eighteenth century, but this disease seems to have been most common on sea voyages. Hospital reform, specifically, an increase in the number of institutions and differentiation of wards by disease, may also have had some effect toward the end of the eighteenth century. See S. Cherry, "The Hospitals and Population Growth," *PS*, 34 (1980): 59–75 and 251–65.

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century, certainly played a role, but it cannot explain the earlier retreat of mortality.[13] Solicitude toward infants, which, according to many historians increased notably around the mid-eighteenth century, may also have affected survivorship, but it has proved difficult to identify changes in infant and child care that reduced mortality rates.[14] Physicians urged frequent washing of children, but parents resisted; wet nursing, which is associated with high mortality, persisted; and state and church intervention established institutions in which abandoned children died in appalling numbers. Although rising incomes in some countries, such as Britain, may have allowed individuals to spend more money on medical services, it is difficult to see how superior services could have been purchased.[15]

If medical care did little or nothing to extend the average life span, some authorities suggest the decline may be attributed to changes in the virulence of microorganisms or in resistance to disease. For example, McKeown and R. G. Record attribute declining scarlet fever mortality in late nineteenth-century England to a change in the virulence of the streptococcus bacillus (but McKeown rejects the idea of a general decline in virulence as an explanation for the mortality decline).[16] Others offer plausible arguments for changes in the virulence of specific early modern diseases, but biological theory suggests that such changes are likely to have occurred in both directions, toward heightened and diminished virulence.[17] Accommodation between hosts and pathogens, which may affect virulence and resistance, is known to be a slow process distributed across thousands of years.[18] Although there is reason to suspect a small underlying increase in resistance, which occurs with familiar diseases in any population over time, this increase would be too slight to account for the dramatic downturn of mortality.

Another body of explanations within this second group points to public health remedies of a different kind than those adopted in the nineteenth century. Recognizing that public authorities in the early modern era wished to sponsor population growth, some historians have suggested that the measures adopted under governmental and private initiative, which Michael Flinn termed collectively

[13] Alfred Perrenoud, *La population de Genève du seizième au début du dix-neuvième siècle: Etude démographique* (Geneva, 1979), 465; and Hofsten and Lundström, *Swedish Population History*, 59. Also see Lee, "Mechanism of Mortality Change," 247–48.

[14] A recent study of this solicitude factor in France, which includes commentary on cause of death, is by Jacques Gélis, Mireille Laget, and Marie-France Morel, *Entrer dans la vie: Naissances et enfances dans la France traditionnelle* (Paris, 1978), esp. 195–227.

[15] Irvine Loudon, "The Nature of Provincial Medical Practice in Eighteenth-Century England," *Medical History* (hereafter, *MH*), 29 (1985): 1.

[16] Thomas McKeown and R. G. Record, "Reasons for the Decline in Mortality in England and Wales during the Nineteenth Century," *PS*, 16 (1962): 17; and McKeown, *Modern Rise*, 16 and 73–90. Also see René Dubos, *The Mirage of Health* (New York, 1959), 143–49. Mirko D. Grmek, *Les maladies à l'aube de la civilisation occidentale* (Paris, 1983), 14–17, proposes study of the *pathocénose*, the disease complex of a given place and period, and antagonisms and symbioses between diseases within it. The *pathocénose* (that is, the mixture of diseases and perhaps also the virulence level) is held to change especially when economic innovations and migrations intrude.

[17] See, for example, Ann G. Carmichael and Arthur M. Silverstein, "Smallpox in Europe before the Seventeenth Century: Virulent Killer or Benign Disease?" unpublished manuscript.

[18] On plague, see J. R. Busvine, *Insects, Hygiene and History* (London, 1976), 55–56; and Frank Nelson Miller, *Pathology* (3d edn.; Boston, 1978), 112.

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"welfare policies," improved public health.[19] Eric Jones and M. E. Falkus detected a decline in the frequency of harvest failures, epidemics, fires, and other catastrophes, which led them to argue that the West detached itself from the East by learning how to control, or avoid, disasters.[20] In general, this body of explanations has failed to identify specific public health measures that might demonstrate growing power in manipulation of the natural environment or in management and avoidance of disease catastrophes. It is all very well to suggest that early modern governments applied quarantines with increasing frequency, but medical historians and historical demographers know the ease with which people evaded these quarantines.

Effective public health measures could nevertheless have operated in the eighteenth century as a result of the efforts of physicians and public health authorities across Europe to reduce the likelihood that epidemics would occur.[21] In the later decades of the seventeenth and early decades of the eighteenth century, a number of internationally renowned physicians, including Thomas Sydenham in England, Bernardino Ramazzini and Giovanni Lancisi in Italy, and Friedrich Hoffmann in Germany, formulated specific measures of intervention. Relying on Hippocratic tradition, specifically, on its suggestion that endemic and epidemic diseases are caused by forces in the environment, and influenced by Renaissance efforts at urban sanitation, these physicians proposed to discover the meteorological and topographical forces that might be blamed for the onset of epidemics. Toward this end, they and their followers embarked on a vast campaign to assemble qualitative and quantitative data about epidemics, climate and weather, geographical and topographical signs, and other features of the habitat. Their aim was to find conjunctures or correlations in the data, occasions when epidemics occurred after the same complex of environmental forces. Early signs of such a complex would offer warnings and allow the adoption of measures of prevention and avoidance. This body of medical theory failed to produce a coherent list of correlations, but it did provide a specific body of measures of avoidance and prevention. The environmentalist physicians proposed to drain swamps, bogs, moats, and other sites of standing water; to introduce hydraulic devices that would circulate water in canals and cisterns; to flush refuse from areas of human habitation; to ventilate living quarters and meeting places and to burn sulfur sticks

[19] Michael W. Flinn, *The European Demographic System, 1500–1820* (Baltimore, Md., 1981), esp. 89–101. Also see George Rosen, *A History of Public Health* (New York, 1958); and, among eighteenth-century populationists, who showed that population could grow through mortality checks, see especially Jean Baptiste Moheau, *Recherches et considérations sur la population de la France* (Paris, 1778); and Johann Peter Süssmilch, *Die göttliche Ordnung in den Veranderungen des menschlichen Geschlechts*, 2 vols. (Berlin, 1761–62).

[20] E. L. Jones and M. E. Falkus, "Urban Improvement and the English Economy in the Seventeenth and Eighteenth Centuries," *Research in Economic History*, 4 (1979): 193–233; and E. L. Jones, "Disaster Management and Resource Saving in Europe, 1400–1800," in Antoni Mączak and William N. Parker, eds., *Natural Resources in European History* (Washington, 1978), 114–36, which also mentions eighteenth-century public health reforms. McKeown and Brown, "Medical Evidence," raised the public health issue in 1955. In *Modern Rise* (p. 153), McKeown held that the question of improvement in environmental conditions remains open but focused on the putative role of nutrition improvement.

[21] The environmentalists' ideas and program are discussed in James C. Riley, *The Eighteenth-Century Campaign to Avoid Disease*, forthcoming 1986.

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or apply other insecticidal measures in houses, hospitals, prisons, meeting halls, and ships; to inter corpses outside the city; and by other measures, including refuse burial, to detach humankind from organic waste. Theory became practice in the 1740s and thereafter as a growing body of physicians and public health authorities persuaded local and central governmental officials, and humanitarians acting on private initiative, to introduce the reforms they recommended.[22]

To have had a significant effect on insect numbers, the measures proposed by the environmentalists would have had to have been broadly applied across western Europe. Two measures, lavation and drainage, are particularly important in insect control, and we can focus on examples of their application.[23] Lavation combines programs taking three forms: flushing filth from urban sites, collecting and disposing of refuse, and introducing devices to agitate or circulate standing water. By these means, which would cleanse streets, industrial sites, and buildings, and transform standing water in canals and cisterns into moving water, the environmentalists argued, the city might be made as healthy as the countryside. One model for these proposals was the naturally washed site of the town of Chester, England, where rain periodically flushed refuse into a subterranean drainage network cleansed by tidal action. The objective of the environmentalists was to introduce the same action by hydraulic engineering. Another model was the program followed in Hamburg to collect and dispose of refuse outside the city each day. A third was the improvement of streets by paving and widening, and of urban drainage networks by constructing or expanding sewage systems. Measures of one or another variety were adopted in many British cities and towns in the Improvement Acts of the 1760s and thereafter, and observers, such as William White in York, attributed declines in mortality specifically to them. In Paris, the drainage system was improved in 1740, and later in the century, other measures, including the emptying of cesspits and the installation of sewers, followed. In the Austrian Empire, Johann Peter Frank directed a broad campaign of medical policing, which included projects for refuse collection and disposal.[24] It is ironic that this part of the environmentalist campaign may have polluted rivers, a possibility of which Leonhard Ludwig Finke warned in urging that Berlin

[22] It is an irony that this campaign is best known in the form of miasma theory. Nineteenth-century environmentalists identified the likelihood of an epidemic onset with odor, railing against stench even after the discovery of the role of pathogens in disease causation. They continued to encourage sanitary reforms, however, which are credited with a role in the late nineteenth-century phase of mortality decline.

[23] Riley, *Eighteenth-Century Campaign*, 91–138, esp. 102–06 and 122–32, gives additional examples of these measures and discusses public health effects of other modes of environmental engineering, citing examples and references.

[24] [John] Haygarth, "Observations on the Population and Diseases of Chester, in the Year 1774," *Philosophical Transactions*, 68 (1778), pt. 1: 131–34; William White, "Observations on the Bills of Mortality at York," *Philosophical Transactions*, 72 (1782): 43; Erna Lesky, ed., *A Complete System of Medical Police: Selections from Johann Peter Frank*, trans. E. Vilim (Baltimore, Md., 1976), 183–89; J[ean] E[mmanuel] Gilibert, *L'anarchie médicinale, ou la médecine considérée comme nuisible à la société*, 3 vols. (Neuchatel, 1772), 3: 18 and 28; Pierre Bertholon, *De la salubrité de l'air des villes, et en particulier des moyens de la procurer* (Montpellier, 1786), 9–77; J. Rawlinson, "Sanitary Engineering: Sanitation," in Charles Singer, *et al.*, eds., *A History of Technology*, 5 vols. (Oxford, 1954–58), 4: 505; C. Fraser Brockington, *A Short History of Public Health* (2d edn.; London, 1966), 21–22; and Jones and Falkus, "Urban Improvement and the English Economy," 197, 212–13, *et passim*.

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authorities suspend disposal of night soil in the River Spree. The action of flushing refuse into water, which reduced the number of urban sites at which insects might breed and feed, may have added to another epidemiological problem, water-borne filth, that made nineteenth-century cities prey to cholera.

The most effective technique for the control of malaria is drainage. Although it is costly, drainage sharply curtails the breeding sites of insects, reduces the incidence of malaria, and brings into use land that is in most cases exceptionally fertile. The economic benefits no doubt explain many eighteenth-century drainage projects, for the individuals who executed them cannot be expected to have invested the sums required to reclaim marshy and sporadically inundated areas for humanitarian reasons alone. Nevertheless, physicians and agricultural reformers encouraged these efforts, and public authorities often facilitated them by tax incentives and direct financial assistance. The most extensively documented projects are those undertaken in France, where tax incentives and decrees facilitating specific projects had encouraged drainage since the late seventeenth century, and where an edict of 1764 renewed and expanded earlier privileges and so inaugurated a vast campaign. In marshy and inundated regions of the coast and the interior, drainage channels were dug, pumps and windmills installed to carry water over higher land, and levees and dikes built to prevent re-inundation. Although partly or completely successful projects were undertaken in many parts of France, especially in river valleys, much of this effort was directed to the densely populated region stretching from the southern coast of Brittany and Poitou in the west to Flanders in the northeast, and directed in such a way that it is reasonable to associate its health effects with both rural and urban areas. Executed by well-capitalized stock companies and by a host of small-scale entrepreneurs, these projects drew on official support to overcome local resistance. André Bourde, who has assembled evidence showing the extensive nature and substantial effect of drainage efforts in France in the decades after 1764, considered the campaign a mixed success. It improved health and gained fertile land but often at the expense of violating communal rights.[25] Newly channeled streams and rivers also aided irrigation in some locales and, in general, enhanced control over inland waters in a way that may have assisted lavation projects. Reformers instituted drainage projects in Prussia and several smaller German states, Scotland, the north and east Midlands and southern England, and Italy.[26]

It is not possible to say how many concentrated breeding sites for insects existed in western Europe or what proportion received treatment. The sources I have

[25] André J. Bourde, *Agronomie et agronomes en France au XVIII^e siècle*, 3 vols. (Paris, 1967), 1: 155–56, 532–36, and 3: 1447–65, 1611–12.

[26] [C. F. L. de La Maillardière], *Le produit et le droit des communes, et les intérêts de l'agriculture, population, arts* (Paris, 1782), esp. 50–80; Harry C. Payne, *The Philosophes and the People* (New Haven, Conn., 1976), 128–29; Clarence J. Glacken, *Traces on the Rhodian Shore: Nature and Culture in Western Thought from Ancient Times to the End of the Eighteenth Century* (Berkeley, Calif., 1967), 487–88 and 657–59; Helen P. Liebel, *Enlightened Bureaucracy versus Enlightened Despotism in Baden, 1750–1792* (Philadelphia, 1965), 99; Lesky, *Medical Police*, 176, 179; and F. Bisset Hawkins, *Elements of Medical Statistics* (1829; reprint edn., Westmead, Farnborough, 1973), 5. F. B. Smith, *The People's Health, 1830–1910* (London, 1979), 421, notes the disappearance of malaria in the north and east Midlands between the 1780s and 1820s.

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consulted—medical geographies and topographies, reformist essays, and medical and public health commentaries—are biased toward the formulation of projects rather than the description of their execution. The environmentalist campaign was broad, and physicians and interested lay people across Europe accepted the reasoning of environmentalists about how epidemics might be avoided or prevented. To uncover the full range of projects implemented requires further work, especially in local history and in the logs and journals of physicians and humanitarians, as well as attention to which reforms were sustained. The evidence now available suggests not a systematic and persistent campaign resulting in complete insect control but instead a campaign consisting of uncounted local efforts waxing and waning in their effectiveness, which, in many individual places and periods, reduced numbers of arthropods and perhaps also rodents below the threshold necessary to cause epidemics. This evidence implies that there was a passage from a regime of insects, in which little or nothing was done to diminish breeding and feeding opportunities, to a regime of local controls. From a national or regional perspective, the overall effect of local efforts was to curtail breeding and feeding opportunities so that the number of insects declined in an oscillating and downward sloping curve. Throughout, the campaign operated on a misconception, for the environmentalists did not understand the role of insects in transmitting certain diseases and, in most cases, did not specifically intend to reduce insect populations.

A CONSIDERATION OF DISEASES AND THE MODES OF THEIR TRANSMISSION can reveal how the process of control probably operated. Recent research has established that the mortality decline is most effectively studied as an age-specific phenomenon, for three reasons: mortality rates of different age groups shifted downward at different times, most of the diseases of particular importance before the mortality decline began affected certain age groups more than others, and the age structure of the European population changed as the death rate fell and the population grew. Pre-decline populations were younger than modern populations, and infant and childhood mortality rates in them were especially high. Many locales experienced infant mortality rates between 200 and 300 per 1000, or higher. This fact escaped full recognition and remains difficult to show in some populations, because infant deaths were underreported. That is, the sources used by historical demographers record births or baptisms in comparatively complete form but fail to report all deaths, especially those in the first months of life.[27] Sources that do not underreport infant mortality (and other methods of estimation) show that high infant death rates were typical of seventeenth-century life and that these rates declined during the eighteenth century, especially after 1740 or

[27] Jacques Houdaille, "La mortalité des enfants en Europe avant le XIX^{e} siècle," in Paul-Marie Boulanger and Dominique Tabutin, eds., *La mortalité des enfants dans le monde et dans l'histoire* (Liège, 1980), 85–118 and Houdaille, "Mortalité des enfants dans la France rurale," 77–106, discuss this point for France and Europe. With some reservations associated with data imperfections, Houdaille accepted that infant and childhood mortality declined in France and Europe, especially from the second half of the eighteenth century.

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1750. Indeed, most of the European mortality decline in its earliest stages can apparently be accounted for by lower mortality rates in the age groups 0–1 and 1–5. Other age groups experienced the decline later.[28]

Until the 1670s, a major factor in European epidemiology and mortality was the bubonic plague. For reasons that remain poorly understood, plague appeared only occasionally thereafter (for instance, in Marseilles in 1720 and in Moscow in 1771). Authorities disagree about how plague figured in overall mortality up to the 1670s. Some believe that it remained a major threat to life, while others hold that it had already ceased to be a leading cause of death. In any case, the infrequency of epidemics of bubonic plague after the 1670s removes plague from consideration as a leading factor in the eighteenth-century mortality decline. This infrequency also removes the rat and the flea from consideration, except insofar as rats and fleas transmitted other diseases of continuing importance.

Identifying other causes of death in seventeenth and early eighteenth-century Europe is at the same time simple and hazardous. Most evidence about causes of death from that period is supplied by lay people rather than physicians, that is, the searchers of the dead in England and Ireland, clerics in Sweden, and others elsewhere whose reports to public authorities were based on inexpert knowledge of cause of death. Piecemeal evidence appears in the case records of physicians and in hospital archives, but this expert testimony suffers from the fact that early modern physicians seldom performed autopsies, willingly gave diagnoses by letter, supplied diagnoses that often have to be interpreted, and believed in a disease continuum according to which the prevailing diseases of one place and time might change character, merging into one another.[29] What is more, investigations into the accuracy of recent clinical diagnoses, in which mid-to-late twentieth-century physicians have a considerable advantage over early modern predecessors, reveal an error rate of up to 50 percent.[30] Early modern sources may be used to identify the most common causes of death, which were often highly age-specific.[31] To go

[28] See Wrigley and Schofield, *Population History of England*; and Houdaille, "Mortalité des enfants dans la France rurale," 77–106.

[29] John Brownlee, "The Health of London in the Eighteenth Century," *Proceedings of the Royal Society of Medicine* (hereafter, *PRSM*), 17 (1925): 73; Saul Jarcho, ed. and trans., *The Clinical Consultations of Giambattista Morgagni* (Boston, 1984), xxvii and l; Dale C. Smith, "Medical Science, Medical Practice, and the Emerging Concept of Typhus in Mid-Eighteenth-Century Britain," in W. F. Bynum and V. Nutton, eds., *Theories of Fever from Antiquity to the Enlightenment* (London, 1981), 122.

[30] It is notoriously difficult, because of the large role of judgment, and because of changes in diagnostic fashions, to detect diagnostic errors. Tests comparing autopsy findings with clinical diagnoses will report high error rates, partly because of errors made and partly because of a tendency to select for autopsy those cases about which diagnosis is least certain. See Iwao Moriyama, *et al.*, "Inquiry into Diagnostic Evidence Supporting Medical Certifications of Death," *American Journal of Public Health*, 48 (1958): 1376–87. In any event, errors persist even when diagnoses have relied on laboratory tests.

[31] Thomas Sydenham, *Opera medica*, 2 vols. (Geneva, 1769); [Jean] Razoux, *Tables nosologiques & météorologiques très-étendues dressées à l'hôtel-dieu de Nîmes depuis le 1er juin 1757 jusques au 1er janv. 1762* (Basle, 1767); and Noah Webster, *A Brief History of Epidemic and Pestilential Diseases*, 2 vols. (1799; reprint edn., New York, 1970). In addition to sources cited in notes 1, 2, and 9 above, see also Charles Creighton, *A History of Epidemics in Britain*, 2 vols. (Cambridge, 1891–94); Thomas R. Forbes, "Births and Deaths in a London Parish: The Record from the Registers, 1654–1693 and 1729–1743," *BHM*, 55 (1981): 371–91; Lars Widén, "Mortality and Causes of Death in Sweden during the 18th Century," *Statistisk Tidskrift* (1975): 93–104; Jan Sundin and Lars-Göran Tedebrand, "Dödlighet och sjuklighet

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FIGURE 1

DISEASES	MODES OF TRANSMISSION
Diarrhea (actually, a symptom, often of dysentery or influenza	
Diphtheria	physical contact, air
Dysentery (amebic and bacillary)	contaminated water or food, insects
Influenza	physical contact, air
Malaria	insects
Measles (rubella and rubeola)	physical contact, air
Plague	insects
Pulmonary Tuberculosis	air
Smallpox	physical contact, air
Typhoid	physical contact, contaminated water and food, insects
Typhus (epidemic and endemic)	insects

SOURCES: See note 32.

beyond that is hazardous. Most causes of death were infectious diseases, and specialists in medical history and historical demography are in considerable agreement about the leading infections. These are listed with their modes of transmission in Figure 1.[32] Most of these diseases are believed to have undergone changes in their incidence between the 1670s, when plague disappeared except for isolated epidemics, and the 1870s. Smallpox remained important through the eighteenth century but then quickly declined. Typhoid declined in significance during the eighteenth century.[33] In England, typhus became less important, declining before disease registration began (in 1838), and thus well before typhus

i svensk järnbruksmiljö, 1750–1875," *Scandia*, 47 (1981): 67–108; Robert Horváth, "The Scientific Study of Mortality in Hungary before the Modern Statistical Era," *PS*, 17 (1963): 187–97; Britt-Inger Puranen, *Tuberkulos: En sjukdoms förekomst och dess orsaker, Sverige, 1750–1980* (Umeå, 1984), esp. 123 and 125; Lebrun, "Les crises démographiques," 205–34; Arthur E. Imhof and Bengt J. Lindskog, "Les causes de mortalité en Suède et en Finlande entre 1749 et 1773," *AESC*, 29 (1974): 915–33; and Øivind Larsen, "Eighteenth Century Diseases, Diagnostic Trends, and Mortality," *SPS*, 5 (1979): 38–54.

[32] This table is based on information from standard sources in pathology, medicine, and medical entomology, including Miller, *Pathology*; Paul B. Beeson, Walsh McDermott, and James B. Wyngaarden, eds., *Cecil['s] Textbook of Medicine* (15th edn.; Philadelphia, 1979); Doland J. Borror and Dwight M. DeLong, *An Introduction to the Study of Insects* (rev. edn.; New York, 1964); Maurice T. James and Robert F. Harwood, *Herms's Medical Entomology* (6th edn.; London, 1969); and Busvine, *Insects, Hygiene and History*. These sources also provide information about insects and diseases discussed below.

[33] Rosen, *Public Health*, 183.

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was distinguished from typhoid in the registers (in 1869).[34] Other infectious diseases seem to have been growing in importance. Among them may be counted cholera, diphtheria, scarlet fever, whooping cough, and perhaps pulmonary and bovine tuberculosis. These diseases figure more prominently in nineteenth-century than in eighteenth-century mortality records (although part of the change must be associated with the difficulty of interpreting eighteenth-century diagnoses). Western Europe experienced both a decline in the frequency and severity of its epidemics—a decline that began in the eighteenth century and has continued to the present—and a shift in leading causes of death away from infections transmitted by contaminated food and insects and toward infections associated with crowding and physical contact (diphtheria, measles, and pulmonary tuberculosis) and polluted water (cholera).

The evidence presented thus far points to the need to find explanations for the eighteenth-century mortality decline that relate innovations introduced after 1740 (when the strongest evidence of decline appears) to innovations that account for lower mortality in the common diseases that had caused heavy losses among infants and children (the enteric diseases, especially dysentery, and malaria).[35] A closer look at the diseases of this era and the role of insects in their transmission is therefore required. Diseases spread by food and those spread by insects are closely linked. If there were grounds for suspecting significant changes in the eighteenth century in the handling, preparation, and preservation of food, then these changes might be assigned some responsibility for the mortality decline. But practices in the slaughterhouses, uncleanliness in the preparation and handling of food, the open-air marketing of food, and similar risk-laden features of alimentation appear to have remained unchanged between 1670 and 1860. One factor that did change was the role of insects.

Some earlier speculations notwithstanding, the importance of insects in disease transmission was not recognized until the late nineteenth and early twentieth centuries. In the space of a few years, largely between 1890 and 1910, scientists detected pathogens on insects or in insect excrement, and the role of the *Anopheles* mosquito in transmitting malaria to humans was proved. Experimenters rapidly developed knowledge of a variety of insect vectors carrying disease to humans, other vertebrates, and plants, knowledge that was summarized in a classic text by William B. Herms, *Medical and Veterinary Entymology*, first published in 1915. These discoveries, communicated to health authorities and concerned lay people often in lurid cries about the dangers of arthropod vectors, evidently provoked a sudden change in attitudes toward certain insects, especially the common fly.[36] Whereas people had previously shown an attitude of friendly tolerance to insects, specialists

[34] McKeown, *Modern Rise*, 70.

[35] Among the diseases with high case-fatality rates (those causing deaths in 10 percent or more of sufferers)—plague, smallpox, typhoid, and typhus—only smallpox is known as a disease to which infants and young children were at particular risk in this period. As already remarked, its role in mortality appears to have declined only in the early nineteenth century. The other three are all associated especially with young adults, although the plague may, before the seventeenth century, have been chiefly a disease of childhood.

[36] See, for example, *The House-Fly at the Bar: Indictment: Guilty or Not Guilty?* (New York, 1909).

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suddenly advised treating them as dangerous pests.[37] For a time, medical entomologists suspected the worst of many insects, believing (incorrectly) that cockroaches transmitted cancer and houseflies tuberculosis. Because diseases carried by insects were already of limited importance in developed countries, aversion gave way to the late twentieth-century attitude of distaste, which seeks to avoid contact with these insects as a matter of comfort rather than chiefly for medical reasons. But in circumstances different from those prevailing in present-day developed countries, populations of flies, mosquitoes, lice, fleas, and lesser vectors may be stupendous. In refuse and filth-strewn habitats, in territories of swamps, bogs, and other sites of standing water, in societies in which no organized effort is made to eradicate insects or even to reduce their numbers, and in appropriate climatic zones, insect vectors have significant health effects. A demonstration of these effects requires an examination of the role of insects in some of the leading diseases of the period 1670–1800.

Amebic and bacillary dysentery are often transmitted through contaminated food (hands contaminated with feces and the pathogens they contain being used to prepare or eat the food). Amebic dysentery is transmitted through contaminated water, also. In bacillary dysentery, the principal agent of contamination is the bacillus *Shigella dysenteriae*; in the case of amebic dysentery, the contaminant is a protozoan found in human feces. Both contaminants are transmitted by several common varieties of flies (house, blow, and flesh flies) and probably by cockroaches. Medical entomologists are reluctant to estimate the separate contributions of contaminated substances and insects as means of transmission. Even though it is generally acknowledged or implied that contaminated food and water constitute the principal modes of transmission in dysentery cases in Europe today, entomologists who have studied dysentery and other insect-transmitted enteric diseases in areas where insect populations are dense stress the role of insects and the probability of food contamination by insects. That is, dense insect populations, especially those of flies, are associated with higher enteric disease levels. Dysentery, in which fatality rates in untreated cases range as high as 50 percent, is especially dangerous to children.[38]

Typhoid is a bacterial infection transmitted from active cases or carriers through food, feces, urine, and by fingers, flies, and fomites. Where flies or other insects play a significant role in typhoid transmission, their numbers are the leading factor. Typhoid and typhus are difficult to deal with in disease history, because in some countries physicians did not distinguish between them before the nineteenth century, and because both produce high case-fatality rates, especially among young adults. Since typhoid fever may be spread by several means, it can persist even though one mode of transmission has declined in significance.[39] A rickettsial

[37] Luther S. West, *The Housefly: Its Natural History, Medical Importance, and Control* (Ithaca, N.Y., 1951), 6–8.

[38] Miller, *Pathology*, 116.

[39] Contaminated water presumably played a role in pre-decline cases, but higher rates of diseases that may have been transmitted by water were not associated with especially densely settled areas, such as the Dutch and Austrian Netherlands, metropolitan London, and northern France, until the cholera epidemics of 1831 and after. Earlier, Europe's human population had evidently been small enough

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disease, epidemic typhus is transmitted primarily by the body louse (*Pediculus humanus humanus*), by means of contaminated louse feces passed into the bloodstream of the human host through the puncture wound of a bite, and much less frequently by other species of lice, the rat flea *Xenopsylla cheopis*, and the rat mite. The louse acquires the rickettsia organism by feeding on an infected person, lives through a stage of rapid multiplication of the rickettsiae, and ordinarily dies in eight to twelve days from physiological damage caused by the rickettsiae. Humans who breathe particles of louse feces suspended in the air can also become infected, since the parasite may live for more than two months in louse feces at room temperature. Endemic or murine typhus is transmitted by several types of fleas and lice, mites and ticks. In the seventeenth and eighteenth century, epidemic typhus was especially common in squalid, crowded quarters such as ships, prisons, and army camps, during the winter and spring. Opportunities for transmission in these circumstances are chiefly a function of the population of rickettsial-carrying lice. The usual case-fatality rate is believed to have been some 20 percent, although the range in adults has been unusually wide: 10 to 100 percent.[40]

Malaria is transmitted solely by the female of certain strains of the *Anopheles* mosquito, which carry several species of "plasmodia," or malaria parasites. Some strains develop the plasmodium but seldom come into contact with people, and in Europe, malaria is known to have been carried by six strains. Case-fatality rates of the most common form in the temperate zone, *Plasmodium vivax*, are low except in infants and children and except among new migrants to an area of endemic malaria. This form of malaria is also known as a debilitating disease, the presence of which augments the likelihood that other maladies will result in death. Its presence increases the risk of contracting certain other diseases as well, especially typhoid and influenza.[41] Because temperate-zone malaria became rare as a direct cause of death before the intensive study of vectors began, it is difficult to reconstruct the historic role of malaria in Europe.[42] This difficulty shows up in the secondary literature in disagreements about the probable range of the carrier in Europe. Some scholars have mistakenly associated mosquitoes with warm climates and not investigated the range of *Anopheles* strains. In fact, recent research establishes malaria as a leading cause of death in the eighteenth century in

that waste finding its way into moving waters was cleansed and purified within the existing conjuncture among volume, distance, speed of water movement, and quantities of waste. The nineteenth-century city, however, produced quantities of filth that could not be cleansed by this natural process. Anne Hardy argues that the Thames was not badly polluted until after 1815. Hardy, "Water and the Search for Public Health in London in the Eighteenth and Nineteenth Centuries," *MH*, 28 (1984): 253.

[40] Miller, *Pathology*, 157; and James and Harwood, *Medical Entomology*, 140.

[41] Mary Dobson, "Marsh Fever—The Geography of Malaria in England," *Journal of Historical Geography*, 6 (1980): 372–76; and Peter Newman, *Malaria Eradication and Population Growth: With Special Reference to Ceylon and British Guiana* (Ann Arbor, Mich., 1965), 13–14, and 77.

[42] Leonard Jan Bruce-Chwatt and Julian de Zulueta, *The Rise and Fall of Malaria in Europe: A Historico-Epidemiological Study* (Oxford, 1980). They hold (p. 112) that the incidence of malaria increased during the seventeenth and eighteenth century and diminished only in the nineteenth, especially after 1850. But their case is based chiefly on reasoning about events likely to have influenced malarial rates (excluding the environmentalists' drainage efforts) rather than on disease records.

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northern England and Sweden.[43] European latitudes and temperatures are not important factors in the existence of this disease, only in its seasonality, which is a function of when vectors carrying the plasmodium are numerous and active. An average isotherm of 60 degrees Fahrenheit, 15.6 degrees centigrade, is sufficient for the activity of most strains of the malarial mosquito, and some strains, *Anopheles claviger*, for example, known to be resistant to cold, tolerate lower temperatures. Furthermore, in northern Europe, some anopheline strains move indoors with the onset of cold weather and remain active into the colder months.[44] Figure 2 distinguishes areas of Europe with a mean air temperature in August above and below 15 degrees centigrade, using data from 1931 to 1960, but does not identify all regions where cold-resistant strains may have been common.[45]

Malaria will disappear below a critical level of vector density. Above this level, it increases in importance as a disease. Thus, the carrier *Anopheles* may be comparatively common without the disease being prevalent, as long as the number of mosquitoes falls below this critical level.[46] In most regions where malaria is common, vector populations fluctuate around the critical level, producing a variable pattern of incidence. The eighteenth-century pattern of malaria, however, remains incompletely known. Although certain eighteenth-century diagnoses are widely interpreted as malaria and others are associated with secondary diseases to which individuals debilitated by malaria are especially prone, some authorities have been reluctant to suggest that malaria may have regressed during the eighteenth century, so long as plausible reasons to suspect such a pattern are missing. Mary Dobson's case study of marshy and non-marshy regions in southeastern England offers the detailed evidence previously lacking. This evidence links the decline of deaths from malaria and secondary infections to drainage and other environmental changes, including larger livestock herds and better-ventilated housing.[47] In the marshy districts, exceptionally high crude mortality rates prevailed into the eighteenth century but began to decline after the

[43] Oiva Turpeinen, "Mortalitetskrisen i Finland åren 1788–1791," *Historisk Tidskrift för Finland*, 66 (1981): 24; Andersen, "Danish Mortality," 9–21; S. T. Anning, "A Medical Case Book: Leeds, 1781–84," *MH*, 28 (1984): 420–31; and Imhof, *Aspekte der Bevölkerungsentwicklung*, 2: 591, 613–18.

[44] William R. Horsfall, *Mosquitoes: Their Bionomics and Relation to Disease* (New York, 1955), 54.

[45] Some small areas in mountains are omitted. The eighteenth-century isotherm of 15 degrees may have been farther south, but numerous sources report malaria at or above the northern limits of the isotherm as of 1931–60. Meteorologists suggest that *Anopheles* needs temperatures over 16 degrees centigrade to breed and that the malaria parasite will not develop at temperatures below 19 to 20 degrees centigrade. See Lamb, *Climate*, 2: 188. Given unequivocal reports (n. 43 above) of malaria cases and epidemics at points above the 19 degree isotherm and around the 15 degree isotherm, these limits seem too narrow. Note the position of Leeds (cited by Anning, "A Medical Case Book") in relation to the line in Figure 2. This figure was drawn by Cathryn Lombardi. Other evidence on this point may be found in J. D. Chambers, *Population, Economy, and Society in Pre-Industrial England* (Oxford, 1972), 98; and Smith, "Medical Science," 123. James R. Busvine reports that anopheline strains known to carry malaria still live in Britain but not in large enough numbers to make malaria indigenous. Busvine, *Insects and Hygiene* (2d edn.; London, 1966), 8. The standard entomological source (Horsfall, *Mosquitoes*, esp. 44–56, 70–73, 92–118, and 122–23) provides extensive data on locale for leading anopheline strains in the three stages—egg, larval, and adult—showing the northerly range of several strains.

[46] Paul F. Russell, "Insects and the Epidemiology of Malaria," *Annual Review of Entomology*, 4 (1959): 418.

[47] Dobson, "Marsh Fever," 357–89.

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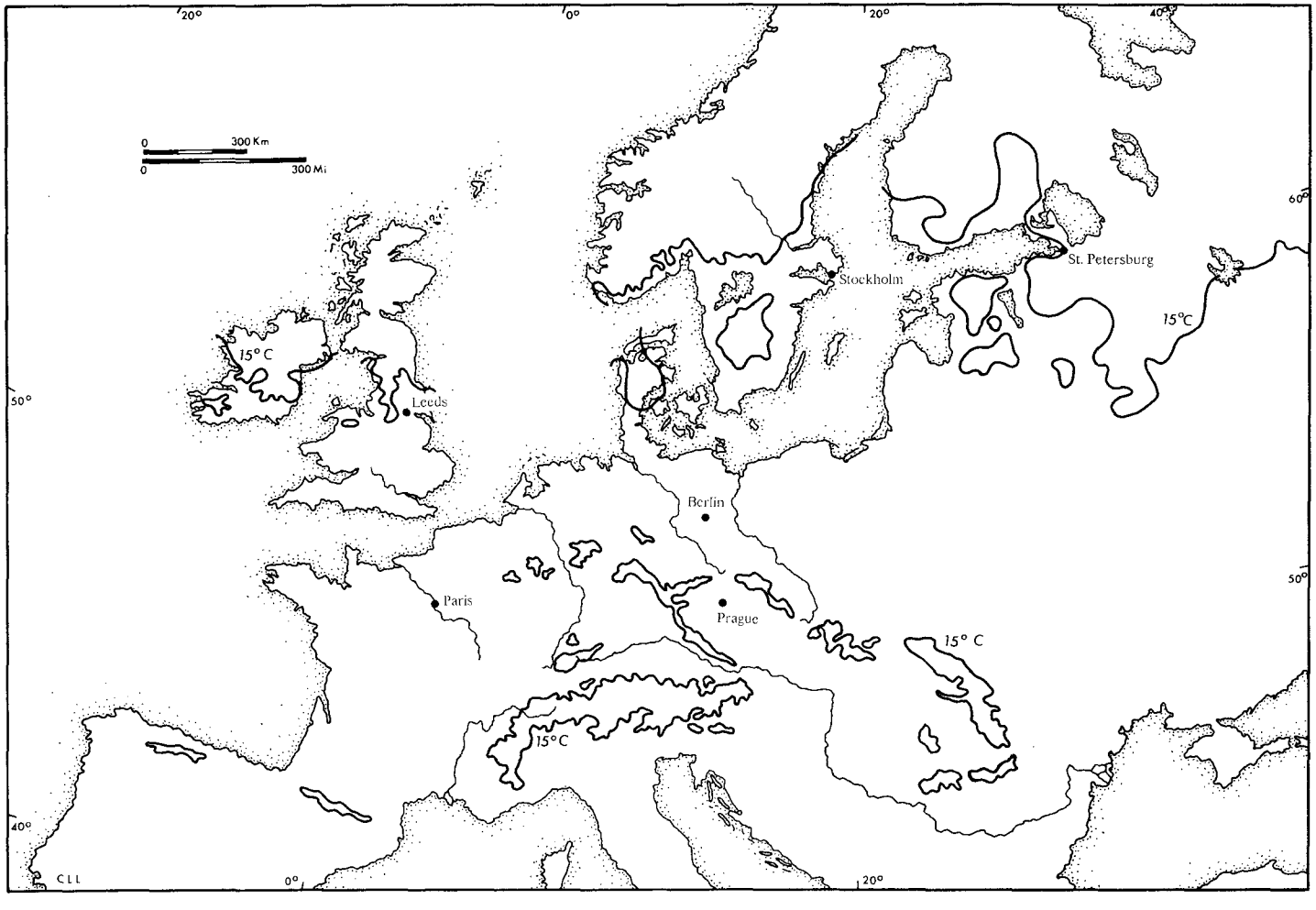


FIGURE 2: Areas of Europe with a mean air temperature above and below 15 degrees centigrade.

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first quarter of the century, especially from the 1740s. For the remainder of the eighteenth century and into the nineteenth, the decline accelerated.

Marshy districts represent an extreme case, in this instance, one in which malaria was severely endemic and associated with mortality rates three or four times higher than those in the healthiest communities of the period and higher even than those in cities. Endemic malaria existed in a much larger portion of the European land mass, including areas under tillage, because some vector strains breed satisfactorily even in small puddles. Although drainage sharply improved the survival rate of people living in marshy areas, the effects acted along a broader front and extended to many areas in which mosquito breeding had been assisted by inundation of the land during part of the year. Across western Europe, the leading concerns caused by malaria were the exceptional mortality risks of marshy areas and the milder direct and indirect contributions to mortality and morbidity in the larger and more densely settled regions in which this disease appeared as one of an array of infections causing death. It is also worth noting that larger livestock herds, which provided optional feeding sites for anopheline mosquitoes, became possible after drainage and were both a cause and an effect of declining malaria. Drainage improved the survival rate of livestock, especially cattle and sheep, even though malaria is not a livestock disease.[48]

Although it is now customary to think of dysentery, typhus, typhoid, and malaria as distinct entities, the problem of concurrent infections complicates the assignment of cause of death for historical populations. Diseases of filth, in particular, are likely to be found simultaneously in given individuals, and these diseases, and malaria, influence one another. Measures that reduce the risk of contracting one or more of them will therefore have a larger impact on health than would be expected from measures affecting maladies that seldom occur together.

Lice are another example of insect vectors. Historical sources contain many indications of louse infestation, as the entomologist James R. Busvine has shown. At times, lice were taken to be a sign of saintly forebearance; those who disrobed the corpse of Thomas à Becket were moved by the large number of lice they found. Samuel Pepys complained of nits in his periwig, George III ordered the heads of his kitchen staff shaved after finding a louse on his dinner plate. If literary sources are a reliable guide, a strong aversion to lice and other body vermin existed before the end of the seventeenth century, among at least a part of the population.[49] Like many insects, lice may be prodigious in number. An infestation of 25,000 to 30,000 lice has been reported on one person. In heavily infested populations, lice typically number 400 to 500 per person. Besides carrying certain human diseases, chiefly typhus but also salmonellosis, lice in quantities approaching these are said to constitute "pediculosis." Meaning simply infestation, this condition is often accompanied by fatigue, irritability, symptoms of illness similar to flu, and a rash like that found in German measles. Lice are most active in feeding and

[48] *Ibid.*, 385 n. 2; and Bourde, *Agronomie*, 1: 536.

[49] Busvine, *Insects, Hygiene and History*, 70–82 and 97–99. Also see Hans Zinsser, *Rats, Lice and History* (1935; reprint edn., Boston, 1963).

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reproducing within their preferred temperature range, which is near human body temperature. Lice may reach large numbers at any season of the year, and people who infrequently bathe and wash their clothes serve as ideal hosts.

Flies that lay eggs in rotting animal matter are "the insects of chief importance in the mechanical transmission of pathogenic organisms."[50] These insects are useful, as they help to cleanse the environment, but they are also harmful, because, while cleansing the environment, they pick up pathogens on their feet or other body parts and carry these disease agents to people. Flies also ingest pathogens and pass them on to people when they feed on human food, regurgitating matter and shedding pathogens carried on the outside of the body. Everyone knows that flies flit about; not everyone knows that a well-fed fly may defecate every four-and-a-half minutes.[51] According to James Busvine, "This kind of insect involvement [in disease transmission] necessitates a large element of chance and no disease germ relies entirely on it."[52] Flies wander over areas several miles square in a largely haphazard search for food. As is the case with other mechanical vectors, the evidence about the fly as a disease carrier is circumstantial. Flies carry more than a hundred species of pathogenic organisms and are believed to transmit more than sixty-five human and animal diseases. But the scale of their role as vectors is difficult to fix from laboratory tests. The infections that flies are believed to spread can also be spread by other means; epidemics of some diseases whose pathogens are carried by flies occur in winter months when flies are dormant, although their occurrence is less frequent in the winter than in the summer. Medical entomologists note statistical associations: heavy populations of flies and factors that encourage reproduction, such as warm temperatures, outdoor markets, open slaughterhouses, and undisposed waste, coincide with the incidence of diseases associated with flies. "Many investigations have shown," concludes Bernard Greenberg, "that outbreaks of diarrheal diseases correspond closely with seasonal rises in the size of the fly population."[53] A reduction of the fly's opportunities to breed, or of its access to human feces, such as occurred with the introduction of waterborne sewage disposal, correspondingly reduces the risk of transmission of disease.[54]

Entomologists gauge the probability of individual cases of a disease carried by a mechanical insect vector, or of an epidemic, by measuring the insect population and attempting to determine levels of pathogenicity among synanthropic flies—those flies that coexist with humans in a close and loosely parasitic relationship (including *Musca domestica*, the common housefly; and *Calliphora vomitoria*, the bluebottle). In general, the effectiveness of an insect vector in disease transmission is a function of the number of vectors and the quantity of pathogens carried. Fly numbers are influenced by temperature, the presence and effectiveness of control

[50] Borror and DeLong, *Insects*, 667.
[51] West, *Housefly*, 264.
[52] Busvine, *Insects, Hygiene and History*, 50.
[53] Bernard Greenberg, "Flies and Disease," *Scientific American*, 213 (1965): 93. Also see West, *Housefly*, 117–18, 121–22, 272–92, *et passim*.
[54] Busvine, *Insects and Hygiene*, 2.

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measures, and access to the human environment. As do rats, flies breed most rapidly where garbage and other waste is left undisposed, where, that is, feces and carcasses are plentiful and accessible. Flies are especially numerous around horse manure and stables.

In a study involving 384,193 flies taken in Beijing, China, researchers estimated that, on average, each fly from a slum area carried 3,683,000 bacteria and each fly from the cleanest district carried 1,941,000.[55] But the issue is not the number of bacteria or pathogens found on a fly. It is whether the flies carry sufficient quantities of viruses, bacteria, or other pathogens to cause sickness. Whereas the bite of a flea infected with plague may deliver from 11,000 to 24,000 bacilli and that of the infective anopheline mosquito thousands of parasites, flies rarely convey large numbers of a single type of pathogen.[56] Bernard Greenberg, examining the role of flies in the transmission of salmonellosis (acute gastroenteritis, enteric fever, and other infections), found that flies do not carry the billion or more bacteria needed to cause the disease in ordinary circumstances. They do carry large enough numbers so that, on deposit at a suitable site such as unrefrigerated meat, the bacteria rapidly multiply to the necessary quantity.[57]

Cockroaches have not been positively incriminated as disease vectors, although the circumstantial evidence is strong.[58] Laboratory tests have shown that cockroaches carry pathogens on their feet and in their feces. It is also known that they feed on human feces and food, and on sleeping persons and corpses. In one study, cockroaches were found to carry a minimum of 13,470 bacteria each, including a wide range of pathogens associated with leprosy, Asiatic cholera, lobar pneumonia, diphtheria, tuberculosis, and dysentery. Cockroaches also thrive in filth and so may be abundant in poor sanitary conditions. In a test conducted in Austin, Texas, 50,000 to 100,000 *Blatella germanica*, a species native to Europe, were found in a single apartment. Nocturnal feeders and nibblers, cockroaches often go unnoticed.[59]

In the twentieth century, scientists gauge the likelihood of an insect-induced increase in disease by monitoring samples of insect populations and levels of pathogenicity in the insects trapped. The probability of an encephalitis

[55] James and Harwood, *Medical Entomology*, 261.
[56] J. L. Cloudsley-Thompson, *Insects and History* (London, 1976), 54–55 and 76.
[57] Greenberg, "Flies," 95.
[58] In addition to the sources cited above, see Louis M. Roth and Edwin R. Willis, *The Medical and Veterinary Importance of Cockroaches*, Smithsonian Miscellaneous Collections, 134 (1958).
[59] Other insects figure in the transmission of diseases, some of which were common and others of which may have been common in early modern Europe. The *Ornithodorno* tick, which may transmit relapsing fever, and which is associated with endemic rather than epidemic manifestations, is European in habitat, although in Europe this disease is usually carried by lice. Russian spring–summer encephalitis and Central European encephalitis are also tick-borne diseases of possible significance in earlier times. Anthrax is passed by horse flies, and the hepatitis B virus is known to be transmitted by bedbugs and mosquitoes, although it is today more often associated with the transfusion of contaminated blood. Insects may also have spread other diseases unrecognized in early modern diagnoses. Parasites transmitted by arthropods (and worms) may have played a role that cannot now be ascertained. Worms deplete cell-mediated immunity, so that a population with extensive worm infestation will have a reduced immunological capacity to resist pathogens.

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epidemic, for example, may be forecast by trapping mosquitoes. Empirical techniques, however, cannot be applied to the past. It is necessary instead to create a metahistory of insect populations. This history should begin with the seventeenth century, when no effective measures of insect control were in use in Europe, when the only sign of antagonism between humans and insects was the early development of an aversion to lice. Common garments, woolen clothes, were not washed or changed frequently, especially in winter. Human beings, particularly those living in crowded quarters, provided an optimal habitat for the proliferation of lice as long as people preferred woolen clothes to linen or cotton. In the seventeenth century, few attempts to drain swamps or other breeding sites of mosquitoes were made, although projects of that type had been undertaken in the sixteenth century for economic rather than medical reasons. Mosquitoes would seem to have bred as freely as allowed by the climate and populations of livestock and humans, the preferred sources of mosquito food. People either did not dispose of their own or their animals' refuse or they collected it for use in agriculture in numerous compost piles, which provided breeding grounds for flies. Horses were replacing oxen as beasts of burden, a change in the composition of livestock populations that was advantageous to the fly. No evidence emerges in the seventeenth century of fly-proof privies or other mechanical devices (such as screens) for reducing the access of flies to breeding sites or to people. Only natural predators of flies, for example, larvae and pupae-eating poultry, spiders, and the ordinary hazards of nature, interfered with their multiplication. Since these conditions also favored other arthropods, the insect population of seventeenth and early eighteenth-century Europe must have been huge. Early entomologists (Charles De Geer, Etienne Louis Geoffroy, Jan Swammerdam, Anton van Leeuwenhoek, and Carolus Linnaeus, among others) attested to the large numbers of insects in descriptive natural histories.

Several problems stand in the way of estimating quantities for a model of insect epidemiology. First, although we can now determine trends, we still cannot readily count insect numbers, and the counts that are reported speak to conditions that do not apply to early modern Europe: they apply instead to numbers of insects in present-day temperate regions where insect control measures are in force, or to tropical areas where control measures may or may not be in force but where insect densities are significantly higher than in temperate zones for most of the year. A second problem arises from estimating the ratio of insects that may be carrying pathogens. Insect populations may rise and fall independently of the infestation of insects with bacterial, protozoan, or viral pathogens, which in many cases do not affect insect longevity. A third problem lies in the realm of estimating the rate at which contacts succeed in transmitting enough pathogens to cause disease. None of these problems can be solved here.[60] To attain some notion of rates of contact,

[60] See George Macdonald, *The Epidemiology and Control of Malaria* (London, 1957), for a mathematical model of malaria epidemiology. Some of the important early statisticians, such as Karl Pearson and John Brownlee, were interested in this issue. See John Brownlee, "The Mathematical Theory of Random Migration and Epidemic Distribution," *Proceedings of the Royal Society of Edinburgh*, 31 (February 25, 1911): 262–89.

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the best we can do is to estimate space and human population densities and guess at insect population densities. Although the human population of Europe in 1700 (about 110 million) was less than one-sixth as numerous as it is today, the insect population, in the absence of controls and of a strong aversion, must have been many times more numerous than it is today. Of course, a sparser human population requires a larger insect population to maintain a given frequency of contact. But insects are capable of prodigiously higher reproduction rates than people are, especially in the conditions that prevailed in seventeenth-century Europe, when waste disposal seldom inhibited insect feeding and breeding, open-air markets and slaughterhouses were common, and people did little or nothing to diminish their contacts with insects.

In an insect regime such as that prevailing in the seventeenth century, diseases would have occurred in a variable pattern associated with insect numbers. Mortality would not be uniformly high but would fluctuate; insect populations would rise and decline. Since they are sensitive to cold, most species suspend activity in the cold months or when not provided with warm hosts. Insect numbers also rise and decline from year to year, whether or not control measures are in force, depending on such factors as the length and warmth of the breeding season and the population size of natural predators. In the seventeenth century, then, diseases transmitted by insects would have occurred together with diseases in which arthropods played no direct role, such as influenza, measles, smallpox, or food poisoning. Once measures of control were inaugurated, insect populations would continue to fluctuate but at lower densities. They would sometimes attain densities at which epidemics again occurred, and the numbers of insects in urban areas would have risen sharply after 1820 because of the breakdown of urban amenities, including environmentalist measures, caused by rapid growth. Under control, however, insect numbers would generally be sufficient to cause some cases of disease, but not to cause epidemics. Other diseases would remain active and might increase in importance to the degree that individuals, especially children, no longer at the same high risk to insect-borne diseases, remained susceptible.

Insect control should therefore be expected to have diminished the numbers and severity of epidemics but to have allowed a variable pattern of mortality to persist. Mortality rates would vary across time and from place to place within the same year, because of the effectiveness of control and the influence of other diseases. These characteristics are compatible with the disease and mortality patterns observed in eighteenth and nineteenth-century Europe, in which variations from place to place and year to year remained a striking feature throughout the first phase of the mortality decline. Available evidence supports the ideas that, first, populations of insects were heavy in the period before the decline; second, insects played a major role in transmitting common diseases, especially those affecting infants, children, and young adults; and, third, eighteenth-century physicians, public authorities, and others introduced and reintroduced measures likely to have reduced the number of places for insects to breed and feed.

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OTHER OBSERVATIONS BOLSTER THE IDEA THAT A SIGNIFICANT PART of the first phase of the European mortality decline can be explained by insect control. It is possible, for example, to compare the recent experiences of areas lacking insect controls with those of areas possessing controls. Because of the seasonal pattern of insect populations in temperate zones, insect control may also be considered by looking at seasonal patterns of mortality.

After the discovery around 1900 of the insect role in disease transmission, scientists attached more importance to the development of chemical means to diminish populations of pests. In the 1930s and 1940s, insecticides proliferated, adding to or replacing compounds and techniques already in use. In the eighteenth century, insect-control techniques of some efficacy had existed side by side with measures having little or no impact. Among the effective substances in use then were alkaloids and sulfur. One procedure was to set sulfur sticks on fire in a closed room infested with insects, a method still regarded as a sound but extreme fumigation technique. Early in the nineteenth century, Europeans imported a Persian mixture called "pyrethrum," the main ingredient of which comes from dried heads of daisy flowers. Pyrethrum causes rapid paralysis in most insects and is still in use in household fly sprays and pet flea-powders.[61]

The creation of new pesticides in the twentieth century made possible the testing of the role of certain insects in disease transmission. Areas that were infested with a particular insect and experienced a significant incidence of a disease could be divided into subregions, one to be treated with pesticides that would reduce or eradicate the insect population, the other to be left untreated. Such an experiment, directed specifically toward assessing the role of the fly as a disease vector, was carried out in Hidalgo County, Texas, in 1946–47, where five of nine towns were treated with DDT and four left untreated. In untreated areas, the rates of death and disease remained unchanged, especially the rates of diarrheal disorders and mortality among infants, whereas in the treated areas, they declined. Twenty months into the experiment, the treatment of the sample towns was reversed at the season in which the fly population could be expected to increase and, after a few weeks, the pattern of infection also turned around.[62] This and other experiments have established a circumstantial case for the role of the synanthropic fly as a disease vector. Similar tests show specifically that, in heavily infested areas, flies transmit enteric diseases, which are especially dangerous to infants. The eradication of the fly is not required in order to reduce contacts between pathogen-carrying insects and humans. Since modern insecticides (if only temporarily) virtually eliminate populations of flies during the period of application, no information is available about the effects of less efficient insect control methods. Presumably, less efficient methods would follow a downward sloping curve of contact probability, which could be a factor of the densities of insect and human populations taken together. But the slope of the curve has yet to be plotted.

[61] Busvine, *Insects, Hygiene and History*, 188, 190–91, and 193. On sulfur fumigation in the eighteenth century, see Boyle Godfrey, *Miscellanea vere utilia: or Miscellaneous Experiments and Observations on Various Subjects* (2d edn.; London, 1737), 142–49.

[62] Greenberg, "Flies," 95–96; and James and Harwood, *Medical Entomology*, 262.

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In temperate zones, populations of insect vectors and certain diseases are seasonal. This coincidence is one of the pieces of circumstantial evidence that links certain vectors (for example, flies) to certain diseases (for example, dysentery). If insect control played a significant role in the decline of mortality in Europe, seasonality in mortality that can be linked to seasonality in insect populations should have declined between the seventeenth and the nineteenth centuries. We must ask first whether mortality in seventeenth-century Europe varied with the seasons, and, if so, whether the variations were associated with diseases transmitted by insects. Second, we must establish which insects were especially likely to multiply in which seasons.[63]

The most detailed study of seasonality was done by E. A. Wrigley and Roger Schofield, who discerned seasonal patterns in burials and in crisis mortality, or deaths during epidemics.[64] In seventeenth-century England, overall mortality showed a strong late summer–early autumn peak only in urban areas, according to the sample used by Wrigley and Schofield, leading them to portray cities and towns as typical sites of fly-borne diseases, such as dysentery, especially affecting children. In crisis mortality, however, a pronounced August–September peak was observed across their sample, a peak higher before 1750 and after 1824 than between those years. Wrigley and Schofield also detected a statistically significant association during the period 1665 to 1745 between higher summer temperatures and higher mortality, with a lag of one to two months. Here, too, the association was weaker after 1746 than before. Shifting the perspective from England to Europe, Wrigley and Schofield observed a weak seasonal peak in overall mortality during the late summer and early autumn in northern Europe and a strong peak in France and regions south of France. The most interesting of these findings deals with crisis mortality, for the environmentalist program was directed specifically at epidemics. The ability of this program to intrude on episodes of peak mortality gives further reason to accept its effectiveness.

In another approach to the problem of seasonal patterns, the medical statistician John Brownlee used the London mortality bills to examine infant mortality since the seventeenth century, taking convulsions in the age group 0–2 as a proxy for infant mortality generally and for diarrheal deaths specifically. Brownlee noted that between 1681–90 and 1750 there was an annual mortality peak located in weeks 30–34 of the Old Style calendar (thus ten, and in the eighteenth century eleven, days later in the New Style calendar). That is, diarrheas peaked in late summer and early autumn (August and September, New Style) up to about 1750, then diminished sharply. The late summer peak showed up again in 1821–40 and persisted until 1902–11. Brownlee related the late nineteenth and early twentieth-century decline of summer diarrhea to the introduction of the automobile and reduction of the horse population and thus of favored breeding and feeding sites

[63] Seasonality has not always been considered by historical demographers or, if considered, has been broken down into monthly or weekly rather than quarterly data and given in long series. Only monthly data serve the present need because the official seasons of the calendar do not coincide closely with annual peaks in insect populations.

[64] Wrigley and Schofield, *Population History of England*, 294–96, 387–98, and 657–59.

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for the fly. He did not offer an explanation for the mid-eighteenth-century decline but noted a strong (although not conclusive) association between peaks in infant mortality and hot summers.[65]

Alfred Perrenoud's study of the population of Geneva provides information on seasonality from the sixteenth to the early nineteenth centuries. Perrenoud noted annual maximums of mortality in winter and late summer and annual lows in July and November, which he regarded as a classic pattern of European historical demography.[66] He also found that the warm season mortality index declined after 1670–99, markedly so after 1700–43, and that the decline was most pronounced among infants and children.[67] This evidence is affirmed by a number of eighteenth-century demographers, who detected clear seasonal patterns in mortality and related those patterns to age-specific death rates. Jacqueline Hecht recently surveyed these findings and showed that, in France, infant and childhood mortality peaked from July or August to October and was associated especially with diarrheas and bacillary dysentery. After 1750, the summer maximum declined in significance and a winter maximum of respiratory diseases replaced it.[68] A similar pattern appears in Arthur Imhof's study of mortality in Scandinavia, with dysentery specifically linked to seasonally high infant mortality during the autumn (August to October).[69]

Both the age and disease-structure of seasonal mortality patterns in the western half of Europe point to maladies transmitted by insects and indicate that these maladies became less important during the period in which environmentalist remedies were introduced. The diseases to which these age and seasonal patterns most apply are gastrointestinal. Malaria, which has a high direct case-fatality rate in children, may also be included because the intestinal localization of malaria produces diarrhea. Diagnoses of enteric diseases may therefore include some cases of malaria but, as a cause of death among adults, malaria is less likely to show a pronounced seasonal pattern because its effects are often deferred. As an infection that lowers resistance to other diseases and leads to chronic ill health, malaria contributes to deaths that may occur months or years after infection. Also, some anopheline strains of mosquitoes move indoors with the onset of cold weather and remain active there into the winter, diffusing a seasonal pattern. The insects specifically implicated in the late summer–early fall mortality pattern are the fly and the mosquito, and perhaps the cockroach. Rates of diseases transmitted by

[65] John Brownlee, "The Health of London in the Eighteenth Century," *PRSM*, 17 (1925): 73–85. Also see Creighton, *Epidemics*, 2: 748–56, linking the high rate of deaths from infantile diarrhea in the early eighteenth century to alcohol consumption among adults.

[66] Perrenoud, *La population de Genève*, 428–29. Pierre Goubert, *Beauvais et le Beauvaisis de 1600 à 1730*, 2 vols. (Paris, 1960), 2: 65, also shows January–February and August–September peaks and a decline in the latter between 1680–1729 and 1771–90. Also see Gélis, Laget, and Morel, *Entrer dans la vie*, 190.

[67] Perrenoud, *La population de Genève*, 429.

[68] Jacqueline Hecht, "L'évaluation de la mortalité aux jeunes âges dans la littérateur économique et démographique de l'ancien régime," in Boulanger and Tabutin, *Mortalité des enfants*, 63–65. Jones, "Decline in Infant Mortality," 245–49, included diarrhea as a cause in urban areas but, observing the seasonal peaks in North Shropshire, held that the decline should be attributed to fewer cases of winter respiratory diseases.

[69] Imhof, *Aspekte der Bevölkerungsentwicklung*, 2: 601, *et passim*.

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these insects peaked at these times, after the insects had had some weeks to rebuild populations from winter lows and become active. The activity and numbers of insects fell sharply in October or, in northern climates, in late September, as temperatures dropped. Mortality peaks during warm weather would be expected to be stronger from France south than from Britain north but were present in all regions in which insect densities are presumed to have been significant in pre-control periods. Mortality from insect-borne diseases lagged behind peak insect populations, marking the interval between affliction with the disease and death. Accordingly, mortality records report deaths after temperatures dropped in the autumn; mortality in the spring has been attributed, in some instances, to malaria contracted in the previous summer.

Louse-borne typhus departs significantly from the seasonal pattern of other insect-borne diseases. Because the lice that find an attractive habitat—a human body—are provided with a temperature range ideal for activity and reproduction, opportunities for contact determine the size of lice populations. Environmentalist measures, specifically chemical antisepsis, were capable of reducing lice populations and opportunities for contact and so may have contributed to the mortality decline. But this contribution probably appeared later, because environmentalists assigned greater importance to measures directly influencing breeding and feeding sites.

In the seventeenth century, Europeans were familiar with an array of diseases now known to be transmitted by several modes, namely, physical contact, air, contaminated food and water, and insects. These diseases, acting sometimes in concert with food shortages, produced variable levels of mortality, which clustered around or above 30 per 1000 per annum. A variable pattern persisted into the nineteenth century, but its peaks, appearing as epidemics, began to occur less frequently and to be less severe. Crude mortality rates declined. The decline appears to have been specific to age, to have begun with lower death rates among infants and children and then gradually to have moved through the age spectrum. Before the decline, mortality had been seasonal, with a peak in late summer and early autumn identified especially with enteric diseases and infant and childhood deaths. The summer–autumn peak lost its leading role during the eighteenth century.

Eighteenth-century environmentalists intruded on this cycle of epidemics. They cleansed the environment, disrupting what they saw as forces external to humans that caused epidemic disease. Their measures—drainage, lavation, ventilation, interment, and others—were not intended to disrupt the breeding and feeding of insects, for the epidemiological function of insects was not understood. These measures constituted an unwitting attack on the insect, an attack that reduced the density of arthropod populations, the key factor in their ability to transmit disease. Around the middle of the century, evidence of declining mortality, especially among infants and children, becomes especially strong. Although mortality remained variable, rates moved progressively to lower average levels until the

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1820s. This progression, like the intervention of the environmentalists, began in western Europe and spread slowly eastward.

Insect control was a significant factor in the European mortality decline. Seasonal peaks in mortality into the eighteenth century are suggestive of the role of insects, but they do not reveal the full effect of insect control. Deaths from diseases transmitted by insects also occurred outside the seasons of greatest insect activity, in the form of primary and secondary infections. Until the mid-eighteenth century, insect vectors, especially the fly and mosquito, had made an important contribution to infant and childhood mortality. Environmentalist measures interfered, curtailing the feeding and breeding of insects, thereby reducing insect populations in most places to a level below that necessary to cause epidemics. These and other remedies, including chemical antisepsis, may have contributed to a reduction in mortality among young adults from typhoid and typhus. The campaign to conquer the environment faltered for a time in the face of nineteenth-century urban growth, which markedly augmented the role of water-borne filth and diseases associated with crowding, and which also aggravated the difference between urban and rural mortality rates. When the campaign was renewed toward the end of the nineteenth century, in the form of public health measures specific to the disease risks of the city, diseases transmitted by insects had ceased to be a leading cause of death. They were the first category of infections to be conquered.

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Historians and the Continuing Controversy over Fair Use of Unpublished Manuscript Materials

MICHAEL LES BENEDICT

ALTHOUGH HISTORIANS REGULARLY QUOTE AND PHOTOCOPY RESEARCH AND TEACHING materials, most are not familiar with the law of copyright. The exceptions are those historians who accept positions as editors of journals or who edit collections of published or unpublished sources. Thus Julian Boyd, the eminent editor of the Thomas Jefferson papers, presented the views of historians and archivists in hearings on copyright revision held in 1965.[1] The rest generally depend on archivists and librarians for guidance when it comes to copying materials, and they depend on publishers for guidance when it comes to quoting materials. But the history of copyright and recent developments in its application to unpublished manuscripts should arouse the interest of the profession. These developments draw into question some of the most common conventions of historical research and writing and could cause historians serious inconvenience, if not worse.[2] Because of the immediate importance of this issue to historians, a review of its history and assessment of the arguments made by those who would restrict scholarly use of unpublished manuscripts is in order.

Authors retain property rights in whatever they write. This "literary property" consists of the actual formulation of the words in which the authors have expressed their ideas. They hold no property rights in the ideas themselves—merely in the particular way they are expressed. A purchaser or recipient may acquire the physical medium in which the words are expressed, but unless the author has transferred the property rights, the author retains control of the literary expression itself. Purchasing a book does not give the purchaser the right to quote its contents; receiving a letter does not permit the recipient to publish it;

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[1] U.S. Congress, House of Representatives, Committee on the Judiciary, Subcommittee no. 3, *Copyright Law Revision Hearings, 1965*, 89th Cong., 1st sess., 1136–44.

[2] The law of copyright, especially the Copyright Law Revision of 1976 and agreements reached thereafter by publishers and educators, is important to historians as teachers as well as publishing researchers, since they deal with photocopying for classroom and research use. It would be well for universities to make available to all instructors sections 107 and 108 of the new Copyright Law, which is chapter 17 of the U.S. Code, as well as copies of the "Agreement on Guidelines for Classroom Copying in Not-for-Profit Educational Institutions with Respect to Books and Periodicals." The latter may be found in numerous places, including Jerome K. Miller, *Applying the New Copyright Law: A Guideline for Educators and Librarians* (Chicago, 1979), 116–19. However, this article deals only with the problem of copyright and fair use of unpublished materials.

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subscribing to a journal does not authorize the subscriber to reprint and distribute its contents, any more than buying a piece of machinery transfers a patent.[3]

Until the passage of the Copyright Revision Act of 1976, American law recognized two types of literary property. The first was made up of the privileges secured to authors under copyright statutes. These statutes originated over 250 years ago in England in response to complaints from the members of the printers' guild, the Stationers' Company. Sixteenth and seventeenth-century decrees and statutes gave these printers a monopoly on the privilege of printing books in England. Although the statutes did not say so, the printers argued that whoever of them first secured the right to print a particular work must be entitled to the right to print all future editions. Outsiders were infringing this "right of copy," they complained.

In 1710, Parliament responded to these complaints with the first English copyright law, which secured old copyrights to printers but placed future copyrights in authors. The law secured copyright to printers and authors only for a term of years, after which, works passed into the public domain. The unhappy printers claimed that this provision could not strip them of the perpetual copyright that vested in them when they acquired and published a work. For fifty years, they struggled in the courts to secure that right. In *Millar v. Taylor* (1769), the judges of the King's Bench finally agreed that the common law did recognize a perpetual right of copy in authors, to which printers succeeded.[4] This copyright continued even after the expiration of the term fixed by the law of 1710, the judges held. But when the case was appealed to the House of Lords, the decision was reversed. According to traditional accounts, the Lords conceded that authors had possessed a perpetual copyright under the common law but that the statute of 1710 had extinguished the right. It became settled law that once a work was published, it received copyright protection only by the terms of the statute; if authors failed to meet its registration or other requirements, they could not turn to the common law for protection.[5]

[3] For general explanations of "literary property" and copyright, see Horace G. Ball, *The Law of Copyright and Literary Property* (Albany, N.Y., 1941), 44–48; Ralph R. Shaw, *Literary Property in the United States* (Metuchen, N.Y., 1950), 12–14; Philip Wittenberg, *The Law of Literary Property* (Cleveland, 1957), 59–62; and Kenneth E. Walden, "Common Law Rights in Literary Property," *Journal of the Patent Office Society*, 37 (1955): 643–48.

[4] 4 Burrows 2303 (King's Bench, 1769).

[5] *Donaldson v. Becket*, 4 Burrows 2408, *Brown's Parliamentary Cases*, 88 (House of Lords, 1774). Howard B. Abrams has shown that the traditional account is erroneous and that it was the judges, whom the Lords had asked for advice, who recognized the existence of a common-law copyright. The Lords themselves, Abrams points out, seem to have rejected the notion. Abrams, "The Historic Foundation of American Copyright Law: Exploding the Myth of Common Law Copyright," *Wayne Law Review*, 29 (1983): 1152–71. For the traditional account of the English background of copyright law, see E. P. Skone James, *Copinger and Skone James on Copyright, Including International Copyright with the Statutes and Orders Relating Thereto and Forms and Precedents* (11th edn., London, 1971), 7–12; Philip Wittenberg, *The Protection of Literary Property* (Boston, 1978), 12–32 and R. R. Bowker, "Development of Statutory Copyright in England," in George Putnam, ed., *The Question of Copyright . . .* (N.Y., 1891), 96–98, which give the orthodox interpretation of *Donaldson v. Becket*. The best account of the origins of the Act of 1710 is in Harry Ransom, *The First Copyright Statute: An Essay on "An Act for the Encouragement of Learning," 1710* (Austin, Tex., 1956).

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In the United States, authors in 1783 persuaded the Continental Congress to recommend to the states that they each enact copyright statutes. All the states complied within three years. A few years later, the framers included a provision for copyright in the American Constitution. Yet, a copyright in effect creates a monopoly, a concept inconsistent with the antipathy of the revolutionary generation to special privileges. For that reason, the framers wrote into the Constitution the justification English jurists previously had articulated for a temporary exception to the anti-monopoly rule: "The Congress shall have Power . . . to promote the Progress of Science and useful Arts, by securing for limited Times to Authors and Inventors the exclusive Right to their respective Writings and Discoveries."[6] In the case of *Wheaton v. Peters* (1834), the Supreme Court confirmed that, in the United States as in England, statutory copyright provided the only protection for published works, and this remains the law today. If an author publishes a work without complying with the registration provisions of the Copyright Act, the work passes immediately into the public domain.[7]

English and American copyright statutes guaranteed exclusive rights of publication to authors and heirs, or those to whom copyright had been assigned, until the copyright term expired. (With some difficulty, the provisions were, over the years, extended to persons working in media other than the written word—artists, sculptors, composers, recording artists, photographers, and the like.) Strictly construed, these exclusive rights meant that no one could in any way distribute copyrighted items of literary or artistic expression to the public without the permission of the copyright holder. However, English and American courts carved out an important exception to the rule: since the purpose of copyright was to promote the well-being of society by advancing knowledge and the arts, other persons—especially scholars and critics—must be entitled to make "fair use" of protected works, even if that meant limited copying and quoting.[8] The courts have never established a fixed definition for "fair use," or "fair dealing," as the English call it. Whether a use has been fair or whether it has infringed copyright has depended on a number of criteria—among which the most important have been the purpose of the use, the nature of the copyrighted work, the amount and substantiality of the portion used, and the effect of the use on the potential market for or value of the copyrighted work.[9]

Until the revision of the American copyright law in 1976, statutory copyright applied only to published works. Until works were published, words of authors were not protected by statute, but by the common law that guaranteed authors the

[6] U.S. Constitution, Art. I, sec. 8; Wittenberg, *Protection of Literary Property* (Cleveland, 1978), 32–34.

[7] *Wheaton v. Peters*, 33 U.S. (8 Peters) 591 (1834); Melville B. Nimmer, *Nimmer on Copyright: A Treatise on the Law of Literary, Musical and Artistic Property, and the Protection of Ideas*, 4 vols. (N.Y., 1985), sec. 4.03; Skone James, *Copinger and Skone James on Copyright*, 4–5; Ball, *Law of Copyright*, 47–48; Wittenberg, *Protection of Literary Property*, 34; and Shaw, *Literary Property*, 29.

[8] Nimmer, *Nimmer on Copyright*, sec. 13.05; Skone James, *Copinger and Skone James on Copyright*, 195–96; Ball, *Law of Copyright*, 259–62; Wittenberg, *Law of Literary Property*, 146–47; and Shaw, *Literary Property*, 67–72.

[9] For general discussions of fair use, see Shaw, *Literary Property*, 67–71; and Wittenberg, *Law of Literary Property*, 146–56. See also the law and cases cited in 17 U.S.C.A. (United States Code Annotated), sec. 107.

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right of "first publication." This was the second type of literary property and sometimes is referred to as "common-law copyright." These rules meant that much of the original source material that historians regularly copy and quote—correspondence, unpublished diaries, memoranda, and business records—was subject not to the provisions of the copyright statutes but to the common law of copyright.[10]

The difference between the legal status of published and unpublished materials posed serious theoretical problems for researchers, librarians, and archivists. First, statutory copyright protected the rights of authors in literary property for only a limited time. The statute of 1710 set the time at fourteen years after publication, renewable for another fourteen years. Present U.S. law limits copyright to fifty years after the death of the creator, replacing the old provision of twenty-eight years after publication, renewable for another twenty-eight.[11] An author's right to first publication of unpublished work, by contrast, was perpetual. Until publication, such work did not enter the public domain, unless the authors or their heirs so dedicated it.[12] Second, the accepted opinion was that the concept of fair use did not apply to materials under common-law copyright. The concept of a right of first publication itself grew out of a combination of concern for property rights and privacy. The opinion in *Millar v. Taylor*, endorsed by the leading early English treatise on copyright, demonstrates how thoroughly the notions were entwined: "It is certain every man has a right to keep his own sentiments, if he pleases; he has certainly a right to judge whether he will make them public, or commit them only to the sight of his friends. In that state, the manuscript is, in every sense, his peculiar property; and no man can take it from him or make any use of it which he has not authorised, without being guilty of a violation of his property."[13] To lawyers, judges, and legal scholars, this opinion suggested that privacy and property rights outweighed the benefits that might be secured to the public by fair use of unpublished materials, and that notion became the established law.[14] So tenacious was the hold of this view that courts had to take real pains to justify even the privilege of a student to take notes of a lecture.[15]

[10] For a basic discussion, see James M. Treece, "Library Photocopying," *UCLA Law Review*, 24 (1977): 1045–49.

[11] *Copyright*, U.S. Code, vol. 17, sec. 302–03 (1978).

[12] The leading American case is *Baker v. Libbie*, 210 Mass. 599, 97 N.E. 109 (1912), which gives the English cases. See Neil Boorstyn, *Copyright Law* (Rochester, N.Y., 1981), 4–6; Ball, *Law of Copyright*, sec. 4; Shaw, *Literary Property*, 27; Wittenberg, *Law of Literary Property*, 76; Walden, "Common Law Rights in Literary Property," 645–47; and William S. Strauss, *Protection of Unpublished Works* (Study no. 29 of *Copyright Law Revision: Studies Prepared for the Subcommittee on Patents . . .*) (Washington, D.C., 1961), 1–5, 32. For a discussion of common-law protection of unpublished private correspondence, see Ball, *Law of Copyright*, 495–500.

[13] *Millar v. Taylor*, 4 Burrows 2303, at 2362 (1769), quoted in Walter A. Copinger, *The Law of Copyright, in Works of Literature and Art* (London, 1870), 6–7.

[14] Skone James, *Copinger and Skone James on Copyright*, 197; Ball, *Law of Copyright*, 260 n. 5; Arthur William Weil, *American Copyright Law* (Chicago, 1917), 115; Shaw, *Literary Property*, 27.

[15] *Queensberry v. Shibbeare*, 2 *Eden's Chancery Reports*, 330 (1758); and *Keene v. Kimball*, 16 Gray (82 Mass.) 545 (1860).

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Thus, under a rigorous interpretation of the law, by quoting unpublished source material, almost every American work of history or biography published in the twentieth century, popular or scholarly, has left its author and publisher open to legal suit for infringement of common-law copyright. It does not matter where the materials reposed, or whether a biography was authorized, because the recipient of unpublished materials is not the copyright holder and therefore cannot transfer copyrights to a repository or scholar. For the same reason, every compilation of manuscripts published under the aegis of the National Historical Publications and Records Commission infringed copyright, as did every microfilmed publication of manuscripts sponsored by the national government or state historical societies. Not only were the libraries and archives that have published such materials liable to suit, but possibly also every library or archive that provided a mechanically reproduced copy of unpublished materials.[16] Of course, there never was any hope of copying and quoting in conformity with a rigorous interpretation of the common law of copyright. That would have meant tracking down the descendants of hundreds of obscure and semi-obscure correspondents and perhaps having to pay them for permission to photocopy and quote. The alternative was for scholars to eschew the benefits of modern reproduction technology and to avoid quoting unpublished primary sources in their published works. The first option imposed a serious burden of time and expense. The second ran against the canons of historical scholarship and style.

In practice, most scholars, librarians, and archivists did not feel constrained by the rigors of the common law of copyright, for several reasons. First, any copyright infringement that did occur raised only a private right of legal action on the part of the copyright holders. The suit could be for damages only. In the absence of special circumstances, these damages would be the financial loss to the holders of the copyright resulting from the copying or quotation. In the case of relatively obscure individuals—probably the majority of those quoted—those damages would be inconsequential and hardly worth the cost of instituting proceedings. In the case of long-deceased authors, the holders of the copyright most likely did not even know that they held it. In the case of well-known figures, the form of copyright infringement—the brief or relatively brief quotation or copying of unpublished materials in the context of a large, scholarly work—minimized the actual loss of value to the copyright holder. Considering all this, before the new law was passed, at least one expert advised scholars to take the risk of copying and quoting, stating baldly, "It will be difficult for an executor to prove damages, and there are no statutory penalties."[17] A leading historian worried that any restate-

[16] For full discussion of the problem of use of unpublished materials, including relevant legal citations, see "Notes—Letters in Need of a Law of Their Own," *Iowa Law Review*, 44 (1959): 705–15. For the effect on unpublished holdings in libraries and archives, see J. L. Wilson, "The Scholar and the Copyright Law," *American Society of Composers, Authors, and Publishers* (ASCAP) *Copyright Law Symposium*, 10 (1959): 104–30; Seymour V. Connor, "The Problem of Literary Property in Archival Depositories," *American Archivist*, 21 (1958): 143–52; Henry Bartholomew Cox, "Private Letters and the Public Demand," *American Archivist*, 28 (1974): 375–86.

[17] Wilson, "Scholar and the Copyright Law," 117.

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ment of the law, even if more generous to scholars than the common law, would discourage the practice by historians of simply ignoring restrictions.[18]

Scholars were less cavalier toward the law when materials that could prove embarrassing to authors or their heirs was involved. Then, it was conceivable that damages might be substantial, as in the case of President Harding's love letters. In that well-known instance, Harding's heirs sued the historian who quoted the letters, which had been acquired from the legal guardian of the recipient. Biographer and publisher were forced to excise all direct quotations of the letters from the forthcoming biography. In part as a consequence of the legal decision, the Harding family was able to secure the letters. After receiving $10,000 from *American Heritage Magazine*, they finally donated them to the Library of Congress, restricting access until 2014.[19] Significant damages could also be sought where a person quoted or photocopied unpublished materials written by well-known individuals or where a history or biography reached a mass market.

The mere fact that law suits were unlikely was not the only consideration that emboldened historians and others to risk infringement of copyright. Ethical people do not infringe the rights of others simply because the chance of being caught and punished is slight. Rather, considering how extreme the traditional view of the law seemed to be, scholars and especially archivists and librarians devised legal rationales for a more flexible interpretation. The common law of copyright was, after all, uncodified and subject to further judicial interpretation. The traditional view seemed patently out of date, based on a solicitude for property rights so passé that it was hard to believe that judges would uphold it without adjusting to the realities of American scholarship. Experts suggested legal arguments to accommodate those realities.

Some analysts argued that making a single copy of an unpublished document or work for the private research use of a scholar could not be construed to violate the common-law right of first publication. "The threat of injury to the copyright owner lies, not in the scholar's use of a photocopy for his private study, but rather in any further dissemination by the scholar of the substance of the copyrighted work," insisted Louis C. Smith, a senior attorney in the Copyright Office.[20] Citing cases that defined "copying" in the legal sense as "the printing of an edition for profit," Ralph R. Shaw also insisted that "copying as used in the common law . . . has never referred to the making of a single copy by a scholar, . . . or anyone else, for his own use. The term . . . applies to public use rather than private use."[21] But

[18] Walter Rundell, Jr., "The Recent American Past and H.R. 4347: Historian's Dilemma," *American Archivist*, 19 (1966): 211.

[19] Francis Russell, "A Naughty President," *New York Review of Books*, June 24, 1982, pp. 30–34. The biography was Russell's *The Shadow of Blooming Grove: The One Hundred Years of Warren Gamaliel Harding* (N.Y., 1968). The suit also cited *American Heritage Magazine*, which had published Russell's "The Harding Papers: How Some Were Saved," *American Heritage*, 16 (1965): 24–31. After the Harding family won a temporary restraining order against publishing the letters, Russell and his publishers, on the advice of counsel, sought an out-of-court settlement that resulted in the payment and transfer of the papers to the Library of Congress. Winn, "Common Law of Copyright," 384. It is fair to suppose that their lawyers warned that they might be assessed significant payments for damages.

[20] Smith, "Copying of Literary Property," *Law Library Journal*, 47 (1954): 206.

[21] Shaw, *Literary Property*, 99.

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these justifications for making photocopies of unpublished materials could not justify quoting unpublished items in published works or circulating them in printed or microfilmed editions of manuscript collections. In fact, these rationales did just the opposite, securing the right to make a copy on the principle that the copied material would not be circulated to the public.

To circumvent the problem, some scholars and librarians argued that donation of unpublished materials to a repository open to the public in itself constituted publication. "The courts have said consistently that placing a copy of a manuscript in a library, where it may be consulted by any member of a broad public, is general publication," Shaw insisted. "If [statutory] copyright is not secured, this publication puts the manuscript into the public domain."[22] Thus, scholars were free to copy and quote without restriction. A serious problem with this justification for copying and quoting unpublished manuscripts remained, however. As already noted, according to the common-law rule, a recipient of a letter or other manuscript received a right only to the physical property and not to the literary property; the recipient acquired no right to publish it. A donor of unpublished materials to a library or archive, for instance, had the right to publish only those materials that the donor had authored. If Shaw and other analysts were correct in saying that to deposit unpublished materials in a library or archive constituted general publication, then the donor was infringing the author's right of first publication by depositing materials written by someone else, and the repository was infringing it by accepting the donation. Shaw recognized the problem. "Scholarly institutions have been concerned about their right to exhibit manuscripts or to make photographic copies of them," he wrote. "The real issue . . . appears to be, whether they have any right to own the manuscript" at all.[23]

"The common law, as it now stands," Shaw wrote in 1950, "is potentially a hand reaching from the grave to stay the advance of knowledge."[24] Recognizing this drawback and also the theoretical problem new photocopying technology posed for copyright in general, scholars, librarians, and archivists struggled with these difficulties into the 1970s. In 1935, representatives of the National Association of Booksellers, the American Council of Learned Societies, and the Social Science Research Council reached a gentlemen's agreement governing the photographic reproduction of published, copyrighted works. By the terms of this agreement, it would be considered fair use for a librarian or archivist to make a single photocopy of copyrighted material for the private use of a scholar. In 1940, the American Library Association adopted a similar position, this time specifically applying it to

[22] *Ibid.*, 139, 142. See also Cox, "Literary Property in Archival Repositories," 148–52. Other experts suggest that deposit of manuscripts in a library or archive constitutes "limited publication"—the distribution of a manuscript to a select group for a limited purpose, without giving them the right of further diffusion or distribution. Winn, "Common Law Copyright and the Archivist," 377–80. Although this rationale might justify photocopying of some materials on the grounds that library patrons are among the select group to whom authors intended to distribute their manuscripts, it would not permit further distribution by quoting the materials in published scholarly works, neither would it change the status of materials in which donors do not hold the literary property rights.

[23] Shaw, *Literary Property*, 139.

[24] *Ibid.*, 18.

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unpublished materials as well as those materials subject to statutory copyright. In England, the Copyright Commission of the British Board of Trade recommended that librarians be permitted to photocopy manuscripts that were over a century old and those whose authors had been dead over fifty years.[25] By the 1950s, archival institutions had also begun to make it a standard practice to require donors of unpublished materials to transfer literary rights to the repositories, with any restrictions deemed appropriate.[26] However, donors could not transfer literary rights that they did not hold.

None of these efforts to extend concepts of fair use to unpublished materials had the force of law in the United States. Proposals to amend the copyright statute to permit fair use of unpublished manuscripts went nowhere.[27] Summing up the law as of 1961, the Register of Copyrights stated flatly that "unpublished works under common law protection are . . . immune from the limitations on the scope of statutory protection that have been imposed in the public interest. These limitations . . . include the 'fair use' doctrine."[28] Or, as another analyst summarized, scholars could make "any use whatsoever of works in the public domain," "*reasonable* use of copyrighted works," and "*no use* whatsoever of unpublished works."[29]

Faced with the disparity between scholarly practice and the letter of the law, historians, librarians, archivists, and other scholars pressed for revision of the Copyright Act. They published numerous articles drawing attention to the problem and posing solutions. They urged that statutory copyright be extended to cover unpublished materials, setting a term after which an unpublished work would enter the public domain. They insisted that the concept of fair use be applied to unpublished as well as published works so that scholars might copy and quote them.[30] Momentum for revision of the law built during the late 1950s and again in the early 1970s, fueled by the concerns of publishers about new photoreproduction technology and by the desire of artists in media other than print to protect their interests. As the Copyright Office and congressional committees assessed the problem, scholars, librarians, and archivists were called on to testify.

The law of copyright was so complex and the interests at stake so varied that it took several false starts and over twenty years of agitation, confrontation, and negotiation to achieve reform. In 1976, Congress passed the Copyright Revision Act, almost completely overhauling the old law. When the dust cleared, it appeared that scholars and their allies had won a great victory. Section 104(a) of the new law expressly brought all unpublished materials under the provisions of the statute,

[25] Smith, "Copying of Literary Property," 197–206; Winn, "Common Law Copyright," 380.

[26] Henry Bartholomew Cox, "The Impact of the Proposed Copyright Law on Scholars and Custodians," *American Archivist*, 29 (1966): 222–23.

[27] For example, Senate Bill 3043, 76th Cong., 3d sess. (1940).

[28] U.S. Copyright Office, *Report of the Register of Copyrights on General Revision of the Copyright Law* (Washington, D.C., 1961), 40.

[29] Wilson, "Scholar and the Copyright Law," 105.

[30] "Notes—Letters in Need of a Law of Their Own"; Connor, "Problem of Literary Property in Archival Depositories"; Cox, "Impact of the Proposed Copyright Law upon Scholars and Custodians"; Cox, "Private Letters and the Public Demand"; Smith, "Copying of Literary Property"; and Wilson, "Scholar and the Copyright Law."

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no matter when or where they had been created. Section 301 preempted all state, common-law jurisdiction, abrogating the common-law copyright in unpublished works, which had recognized no right of fair use. Like published materials, unpublished works would be entitled to statutory copyright protection from the time of creation until fifty years after the death of the creator. To compensate holders of the common-law right of first publication for being stripped of that perpetual right, the law provided that in no case would an unpublished work pass into the public domain until the year 2003.

Although it may seem strange that unpublished letters from the father of a prospective novice to the abbot of a thirteenth-century monastery should be protected by American copyright until 2003, other provisions of the 1976 law seemed to minimize the burden placed on scholarship. The section of the law defining publication omitted any reference to donation of materials to a library.[31] This omission effectively eliminated Shaw's argument that such a donation constituted publication. Even though this meant that a donor's unpublished work could not be construed to have entered the public domain, it also vitiated any fear that a repository violated copyright by accepting unpublished materials from a recipient.[32] Much more important, section 107 of the law for the first time gave statutory recognition to the doctrine of fair use, citing the criteria to be applied but leaving the courts to apply them on a case-by-case basis. Section 108 laid down specific guidelines for photocopying by libraries and archives open to the public. Subsections 108(a) and (b) permitted such libraries and archives to make a single photocopy of a published or unpublished work as a backup for security, preservation, or for research use in another library, when replacements could not be acquired at a fair price. Subsection 108(d) permitted such libraries and archives to provide single photocopies of parts of a work for private, scholarly use. Subsection 108(e) permitted similar photocopying of large portions or entire works for scholarly use, if reasonable investigation indicated that a copy could not be obtained at a fair price.[33] The law required repositories to display prominently a warning to users about their obligations to honor copyright. Subsections 108(d), 108(e), and 108(g)(2) confirmed the right of libraries to make photocopies for interlibrary loan (so long as they did not do so in quantities that would substitute for subscriptions or purchases) and to deposit photocopies of research material in other repositories.

THE COPYRIGHT REVISION ACT OF 1976 DID NOT SECURE EVERYTHING THAT SCHOLARS may believe necessary to foster research in unpublished materials. Although the law confirmed the right of repositories to deposit photocopies of otherwise unavailable material in other libraries and archives, it is doubtful whether this right

[31] *Copyright*, U.S. Code, vol. 17, sec. 101 (1978).

[32] It could be argued that the phrase "or other transfer of ownership" among the specific acts that constitute publication under the law could include donation to a repository, but it is not persuasive. The concept is well known and the issue of libraries and unpublished materials had been discussed before Congress passed the law. It would have been easy to specify donation to a public repository if Congress had so desired.

[33] The actual language specifies several further conditions not relevant to this discussion.

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would permit the publication of microfilm editions of manuscript collections, such as those sponsored by the National Historical Publications and Records Commission and by state historical societies. Certainly, publication of printed editions of unpublished correspondence, speeches, and the like will infringe copyright until at least 2003. Still, the law seemed significantly to ease the limits that the common law imposed on scholarly use of unpublished materials. The required copyright warnings immediately sprouted over xerox machines in libraries and archives, so that patrons could take advantage of the liberal provisions governing photocopying for scholarly research. The Society of American Archivists (SAA) issued a statement on the reproduction of manuscripts and archives for reference use that proceeded on the assumption that such reproduction was consistent with the law.[34] SAA-sponsored practice manuals carefully explained the provisions of sections 107 and 108, and archivists describing the applicability of copyright to unpublished materials expressed relief that the law "removed cause for . . . confusion" about their status.[35] It seemed clear that scholars could now make and secure photocopies of unpublished materials to forward their research and could quote unpublished sources in their scholarly publications. Libraries and archives could distribute microfilmed copies of their manuscript collections to other repositories for research use and could provide copies through interlibrary loan.

But, gradually, a few hints appeared that all was not well. Analysts expressed doubt about the application of fair use to unpublished materials even under the new law.[36] They worried that a liberal application of the law to unpublished materials invaded privacy.[37] The staff of the Copyright Office appeared not to share the views of archivists about the applicability of the law's fair use and photocopying provisions to unpublished materials. It is "a tough little issue," one staff member responded to an archivist's query.[38] An American Library Associated-sponsored explication of the law stated flatly, "One may not copy these materials without the permission of the author or his or her heirs. The author's 'right of first publication' takes priority over all other considerations."[39]

The potential for serious trouble became clear in January 1983, when the Copyright Office published a report on *Library Reproduction of Copyrighted Works*.[40] In that report, the staff pointed out the paucity of the legislative record and of expert comment concerning unpublished manuscript materials. In addition, the staff quoted the legislative history that accompanied the bill, which stated that "the

[34] *American Archivist*, 39 (1976): 411.

[35] Sue E. Holbert, *Archives and Manuscripts: Reference and Access* (rev. edn.; Chicago, 1977), 17–18; Carolyn Hoover Sung, *Archives and Manuscripts: Reprography* (Chicago, 1982), 52; "SAA Statement on Copyright," *Society of American Archivists Newsletter*, July 1980, 2–4; and Michael J. Crawford, "Copyright, Unpublished Manuscript Records, and the Archivist," *American Archivist*, 46 (1983): 139.

[36] Jane Tucker Dana, "Copyright and Privacy Protection of Unpublished Works—The Author's Dilemma," *Columbia Journal of Law and Social Problems*, 13 (1977): 373–77.

[37] *Ibid.*; and Alan Lee Zegas, "Personal Letters: A Dilemma for Copyright and Privacy Law," *Rutgers Law Review*, 33 (1980): 134–64.

[38] Holbert, *Archives and Manuscripts: Reference and Access*, 18.

[39] Jerome K. Miller, *Applying the New Copyright Law: A Guide for Educators and Librarians* (Chicago, 1979), 22.

[40] U.S. Copyright Office, *Report of the Register of Copyrights: Library Reproduction of Copyrighted Works (17 U.S.C. 108)* (Washington, D.C., 1983).

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applicability of the fair use doctrine to unpublished works is narrowly limited."[41] Apparently influenced by concerns about privacy and displaying a lawyerlike desire to conserve old doctrine, the staff interpreted the law so as to reverse explicitly or implicitly the gains scholars thought they had made under the revision of 1976.[42]

First, the staff of the Copyright Office insisted, only subsection 108(b) of the photocopying provisions applied to unpublished manuscripts—the subsection permitting repositories to make a single photocopy of a work for internal preservation and security use. Subsections 108(d) and 108(e), which govern interlibrary loan and photocopying for researchers, applied only to published works, the staff averred.[43] Acting on this conviction, the Copyright Office proposed to eliminate any ambiguity by recommending that Congress amend subsection 108(e) so that it would explicitly apply to published works alone.[44] Second, citing the statement in the legislative record that fair use of unpublished materials "is narrowly limited," the staff insisted that "there is *no* fair use copying permitted beyond that authorized by subsection 108(b)."[45] Because copying for research purposes has always been considered a fair use of copyrighted material, the implication was that the staff did not believe that the fair-use section of the law (section 107) applied to unpublished materials at all. If the staff were right, scholars would infringe copyright if they quoted unpublished materials before they entered the public domain. That is, historians could quote no unpublished source, no matter how old, and no matter what the nationality of the author, until 2003. Even after that, historians would still infringe copyright if they quoted an unpublished source within fifty years of the death of the author. In sum, the interpretation proposed by the Copyright Office would make it an infringement of copyright to quote unpublished materials in books or articles, to provide photocopies of unpublished materials to scholars, to circulate photocopies of collections through interlibrary loan, or to deposit photocopies for research use in other libraries or archives. In fact, if the report's interpretation were accepted, it would leave scholars only with the improvement that unpublished materials will come into the public domain eventually; it would gut everything else.

For most historians, acceptance of the view of the Copyright Office would mean that they would not be able to photocopy, quote, or obtain access through interlibrary loan to unpublished sources until 2003. An even longer time would elapse before a scholar could photocopy and quote materials written in the first part of the twentieth century. Most scholars active today would be dead before they would be able to quote or photocopy a private letter written by a thirty-five-year-old correspondent to, say, Felix Frankfurter in 1936. If the correspondent lived to be

[41] *Report of the Register of Copyrights*, 105–06.

[42] In the staff's discussion of the relevance of the photocopying provisions of the law to unpublished manuscripts, it consistently refers to authors as having "chosen" not to publish, suggesting a conscious decision by authors to maintain the privacy of such materials by shielding them from public view. *Ibid.*, 105–06, 121.

[43] *Ibid.*, 104–06, 121–22, 123–24.

[44] *Ibid.*, 329–31.

[45] *Ibid.*, 106.

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seventy, his letter would come into the public domain in 2021. For future scholars of recent history, the result would be devastating. In 2003, when most materials of interest to others will have become available, these scholars or the librarians and archivists serving them would infringe copyright if they quoted, photocopied, or secured through interlibrary loan the letters, memoranda, diaries, or notes of anyone who died after 1953, unless the authors had agreed to transfer that right to the public or to the institution holding their papers. (And donors could not transfer the rights of authors whose letters or other materials they had received.) Given the average lifespan of American men and women, these restrictions would mean that, in 2003, a scholar would not be able to quote, photocopy, or secure through interlibrary loan the unpublished writing of most people born since 1890.

Of course, the same practical considerations about the real danger of a lawsuit over copyright infringement pertain under the new law as under the old common-law rules. It would remain unlikely that the heirs of an obscure or relatively obscure correspondent would go to court to protect their rights. However, by reviving the old common-law rule that fair use does not apply to unpublished manuscripts, the report of the Copyright Office would confirm the holdings of the Harding love letters case. Since section 503 of the revised law authorizes the courts to impound and destroy all copies of works that infringe copyright (irrespective of actual pecuniary damages), acceptance of the report would place a powerful weapon in the hands of those seeking to suppress publication of embarrassing historical materials.

In one recent example, John Belushi's wife, Judith, sought to have impounded all copies of Bob Woodward's unflattering biography of her husband, *Wired: The Short Life and Fast Times of John Belushi* (1984), on the grounds that Woodward without her permission included photographs from Belushi scrapbooks, violating her copyright on the previously unpublished materials. With a bestseller at stake, the publisher, Simon and Schuster, resisted firmly and was forced only to agree to remove the photographs from future printings.[46] But I cannot help but wonder whether smaller publishers, with fewer sales at stake, would offer so stout a defense. If fair use does not apply to unpublished materials, the same situation could arise whenever a researcher consulted private papers and then quoted them without first securing permission from the copyright owner.

What might be more inhibiting than rare lawsuits would be the knowledge that the old ethical justifications for risking legal action no longer pertained. Acceptance of the report would explode the rationalization that traditional views of common-law copyright were so unreasonable as to be inapplicable to modern scholarship. The report is a point-blank reaffirmation of the most rigid interpretation of the common-law rules and proposes to engraft those rules on the statute. Scholars still might be willing to take the risk of infringing copyright, ignoring or somehow rationalizing the affront to ethical standards of knowingly violating someone else's rights. But would librarians, archivists, and publishers do the same? It is at least doubtful. Libraries, archives, publishing houses, and other institutions

[46] *Belushi v. Woodward*, 1984 Copyright Law Decisions, para. 25,558 (D.D.C. 1984) (no. 84–1628).

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must establish formal, written guidelines governing these matters. It is not likely they would establish guidelines that openly flouted the law. The risks would be greater for these institutions than for individual scholars. A scholar may publish a few books or a number of articles. The chances of a copyright holder taking a scholar to court are slight. A publisher publishes a large number of books, a journal publishes a great number of articles, a library or archive photocopies thousands of unpublished documents. The risk for them is greater. These institutions, not the individual scholar, would ultimately make the decision about how to comply with the law.

At present, neither repositories nor publishers have changed their guidelines to conform to the strictures in the report of the Copyright Office, judging from responses to queries sent to a select group. Nearly every repository that responded disclosed that it has interpreted the photocopying sections of the law to apply to unpublished materials. However, few of the responding repositories and publishers have provided explicit guidelines about quoting unpublished sources. Of the repositories, six indicated explicitly or implicitly that users could quote unpublished sources, while one stated that they could not. Of the publishers, two signified a belief that fair use applies to unpublished materials, while one stated that it does not.[47]

The least serious consequence of accepting the report of the Copyright Office would be that those who write history might have to stop quoting from most of their sources until those sources enter the public domain. More serious would be the burden of effort, time, and therefore expense involved in eschewing the modern technology of photoreproduction. It would mean longer stays in libraries and archives and visits to fewer of them. At the same time, scholars would be unable to consult copies of unpublished materials secured through interlibrary loan or through permanent acquisition for their libraries. It would mean less thorough research or a longer time and greater expense before research was completed and results published. It would suggest exchanging in scholarly research the technology of the twentieth century for that of the twelfth. Such changes as these seem so inconceivable that it is hard to imagine that anyone would purposely impose them. In fact, these would be the largely unintended consequences of decisions

[47] I sent queries to the leading state historical societies, other leading manuscript repositories, and the presidential libraries associated with the National Archives. Eighteen responded. Of these, thirteen indicated explicitly or implicitly that they permitted photocopying under the provisions of the copyright statute, while the policies of five were unclear. Responding repositories were the American Antiquarian Society, the Church of the Latter Day Saints, the California Historical Society, the Eisenhower Library, the Ford Library, the Houghton Library of Harvard University, the Huntington Library, the Johnson Library, the Manuscripts Division of the Library of Congress, the New York Historical Society, the New York Public Library, the Ohio Historical Society, the Historical Society of Pennsylvania, the Southern Historical Collection of the University of North Carolina, the Texas State Historical Society, the Truman Library, the Virginia Historical Society, and the State Historical Society of Wisconsin. Queries were sent to those presses that took two pages of advertising in the program book of the 1984 convention of the American Historical Association. Responding publishers were the Dorsey Press, D.C. Heath, Houghton Mifflin, Johns Hopkins University Press, Princeton University Press, Random House–Alfred A. Knopf, Stanford University Press, University of California Press, University of Illinois Press, University of North Carolina Press, University Press of Kentucky, and Yale University Press.

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designed to protect authors from abuses by others. Once the burdens that such restrictive interpretations placed on scholarship became clear to legislators, they would no doubt make efforts to ease the rigor of the law. But, in the meantime, confusion would continue about the rights and obligations of scholars and repositories, and no one would know how accommodating to scholarship yet another revision of the law would be.

The report of the Copyright Office on library reproduction of copyrighted works was mandated by the terms of the 1976 revision act.[48] In this law, Congress instructed the Copyright Office to propose such changes as it deemed warranted, and the implication is that Congress will consider them. As yet, the Copyright Office has exerted no pressure for such consideration, and informed historians will no doubt allow the report, with its recommendation for congressional action, to slumber as long as possible. However, the report of the Copyright Office attended to a broad range of issues related to photocopying, some of which are of intense interest to authors, publishers, and librarians. It is possible that pressures may build for modifications in the law based on the report, and, in that case, Congress may take under consideration the report's interpretation of fair use of unpublished materials and its recommendation that Congress embody that interpretation in the statute. After all, while the proposal may seem extreme to historians, it does not seem so to the staff itself or to representatives of authors. In fact, the proposal conforms to the present British copyright law.[49]

Developments in the courts have reinforced the drift toward a restrictive interpretation of the application of fair use to unpublished materials and have made ultimate acceptance of the Copyright Office report more likely. These developments have occurred in connection with the important case of *Harper and Row v. Nation Enterprises*.[50] In the *Harper and Row* case, former President Gerald Ford sold the rights to his memoirs to Harper and Row. In turn, the publisher sold the first right to publish excerpts from the memoirs to *Time* magazine, delivering a pre-publication copy of the manuscript that would enable *Time* to be the first to report on its contents. Despite tight security, the *Nation* secured a stolen copy of the manuscript and published a brief report of key sections, quoting from the manuscript and "scooping" *Time*. *Time* backed out of its contract with Harper and Row, which in turn sued the *Nation* for damages.

The case raises some of the thorniest issues of journalistic ethics. There can be no question that Ford's inside information and views on a variety of events were news and that newspapers, magazines, and television would report widely on them. *Time* had technically purchased only the right to quote the memoirs, but in fact what it had tried to purchase was the ability to triumph over the competition by securing the manuscript prior to publication. What was important to *Time* was not the right to quote but the ability to be first to report on what the memoirs revealed, as demonstrated by the refusal of its editors to honor their contract with Harper

[48] *Copyright*, U.S. Code, vol. 17, sec. 108(i) (1978).
[49] Skone James, *Copinger and Skone James on Copyright*, 272–73.
[50] *Harper and Row v. Nation Enterprises*, 557 F. Supp. 1067 (S.D.N.Y. 1983), 723 F.2d 195 (2d Cir. 1983), 105 S.Ct. 2218 (1985).

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and Row, once the *Nation* had published its report. What *Time* had tried to do, in effect, was purchase the right to the news.[51] Although *Time*'s actions and the *Nation*'s response may have raised the issue of "checkbook journalism" for the news media, to publishers, copyright lawyers (especially those representing publishers), and many authors, the *Nation*'s actions amounted to flagrant literary piracy. After all, the *Nation* also wished to be first to publish a report of the memoirs, and it had secured a stolen copy of the manuscript to do it. The *Nation*'s editors may even have paid for the stolen manuscript, although nowhere is the charge made explicitly.

The *Nation* relied on the doctrine of fair use in its defense. The publication of the Ford memoirs was a legitimate news event, the *Nation*'s lawyers argued. The magazine had quoted only tiny fragments of the whole manuscript, and the quotes made up only a small part of the *Nation*'s story. Such limited quotation for the purpose of reporting a news event surely fell under the scope of fair use, they insisted. The district court, plainly outraged at the piracy, found for Harper and Row. On appeal, the circuit court reversed the decision, arguing that the First Amendment mandated a preference for a free press over property rights.

In the lower courts, neither Harper and Row's lawyers nor the judges attended to the fact that the report in the *Nation* appeared before the Ford memoirs were actually published. The case turned on whether the *Nation*'s quotations from the manuscript constituted fair use. The publication status of the Ford memoirs seemed irrelevent to all concerned. Everyone seemed to conclude implicitly that the Copyright Revision Act eliminated the distinction between published and unpublished works when it came to applying the doctrine of fair use. With the *Nation*'s victory on appeal, however, both legal commentators and Harper and Row's lawyers suddenly discovered the relevance of the manuscript's status. This issue became one of the three questions Harper and Row's counsel presented to the Supreme Court for review.[52] In *amicus curiae* briefs sustaining Harper and Row, both the Association of American Publishers and the Volunteer Lawyers for the Arts suggested, as the latter worded it, "The unpublished nature of President Ford's manuscript is the hinge on which this case should turn."[53] Legal scholars echoed the briefs.[54]

Harper and Row's counsel and their supporters dealt only with the quotation of unpublished works and not with photocopying, and they did not go as far as the Copyright Office staff did in limiting the applicability of fair use. They

[51] The right to quote Ford's memoirs, separate from the right to quote first, could be valuable only if other news media would not have the right to quote Ford's memoirs even in post-publication reports—that is, only if such quoting would not be fair use. However, neither Harper and Row nor the courts have made any such suggestion.

[52] Brief of Harper and Row, Pub., Inc. and the Reader's Digest Association, Supreme Court, no. 83–1632, p. i.

[53] "Brief Amicus Curiae of the Association of American Publishers, Inc., in Support of Petitioners," *Columbia Journal of Arts and the Law*, 9 (1985): 264–79 (hereafter, Association of American Publishers, "Brief"); "Brief Amicus Curiae of Volunteer Lawyers for the Arts, Inc., in Support of Petitioners," *ibid.*: 281–85 (hereafter, Volunteer Lawyers for the Arts, "Brief"). The quotation is on p. 282.

[54] David B. Goroff, "Fair Use of Unpublished Works: Harper and Row v. Nation Enterprises," *Columbia Journal of Arts and the Law*, 9 (1985) 325–50; James Mandelbaum, "The *Nation*: Overprotection of the First Amendment in Fair Use Analysis," *Journal of the Copyright Society*, 32 (1984): 154–55; William F. Patry, *The Fair Use Privilege in the Copyright Law* (Washington, D.C., 1985), 123–42, *et passim*, 448.

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reluctantly conceded that the law put an end to the common-law exemption of unpublished materials from the doctrine of fair use. But they insisted that the publication status of protected material is one of the criteria to be considered in establishing whether an unauthorized use has been fair and that such a use can be fair only in "extraordinary circumstances."[55] At first, this reading seemed more generous to scholars than the Copyright Office's more restrictive interpretation, but, in fact, the effect of a rule permitting unauthorized use of unpublished materials only in extraordinary circumstances well may be the same. If the meaning of "extraordinary circumstances" requires that scholars demonstrate that their use will fulfill a specific, compelling need, it is hard to see how ordinary scholarship could qualify. Scholars could argue, however, that if a host of facts suggest that an author did not desire to shield his or her words from publication for the full copyright term, this circumstance would be extraordinary enough to justify fair use. Likewise, scholars might well argue that utilizing a seventeenth-century manuscript, for example, is extraordinary compared to the usual case that raises copyright issues. The scholar may feel justified in quoting or copying that material, despite its protection under the new law, especially given the small likelihood that anyone would take legal action. But scholars working with recent materials, on controversial or embarrassing subjects by which someone might feel aggrieved, would run a more serious risk of legal action. Even for scholars not so burdened, acceptance of the restrictive view would mean a return to rationalizations about their right to quote and copy unpublished materials. Again, much would depend on the decisions of publishers and repositories rather than on those of scholars themselves.

The *Nation* and its allies seem to have been caught unprepared by Harper and Row's argument that fair use applied more narrowly to unpublished than to published manuscripts. Their response was feeble—simple reliance on the section of the law preempting state, common-law jurisdiction, with no further argument. Perhaps as a consequence, in May of 1985, the Supreme Court found for Harper and Row, with part of the 6–3 decision turning on the majority's conclusion that fair use was of limited applicability to unpublished materials. The case did not involve photocopying under section 108 of the law, and the justices did not go so far as to limit fair use of quotation of unpublished materials under section 107 to "extraordinary circumstances," but they did cite several of the recent treatises noted above to conclude "that the unpublished nature of a work is '[a] key, though not necessarily determinative, factor' tending to negate a defense of fair use."[56] Exulting in the Supreme Court's sympathy for the rights of authors and publishers, the publishers' copyright attorney, Irving Horwitz, urged an attack on the doctrine of fair use itself as "a loophole that publishers should seek to close up." This remark led even Irwin Karp, the attorney for the Authors' League and no friend of liberal interpretations of fair use, to object that "without fair use many

[55] Volunteer Lawyers for the Arts, "Brief," 284; Association of American Publishers, "Brief," 266; Goroff, "Fair Use and Unpublished Works," 347; and Patry, *Fair Use Privilege*, 126.
[56] *Harper and Row v. Nation Enterprises*, 105 S.Ct. 2227–23.

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historians and authors would not be able to put pen to paper." Horwitz responded, "Scholars are authors and users but also abusers."[57]

As of 1986, then, it is very possible, perhaps probable, that the law of fair use of unpublished materials may return to something very close to what it was before Congress passed the Copyright Revision Act of 1976. In fact, the most recent explication of law for archivists has returned to the old advice: archivists and researchers should take the risk of infringing copyright in unpublished materials, since the chances of legal action are remote.[58] Concerned about the erosion of scholarship's gains, archivists have been discussing how to resist the trend. A session devoted to the Copyright Office report at the 1984 convention of the Society of American Archivists was heavily attended, with presentations offered from the perspectives of archivists, historians, librarians, and the Copyright Office.[59]

ALTHOUGH RESTRICTIONIST ARGUMENTS HAVE MADE HEADWAY, SCHOLARS MAY HOPE to reverse the trend, since the restrictive reading of the fair-use and photocopying provisions of the 1976 law has been based on a series of propositions that are not persuasive. The arguments have been suffused with the view that unpublished materials are inherently different from published materials and must be treated differently. But, in law, the difference between the treatment of published and unpublished materials has always been based solely on the inapplicability of statutory copyright to unpublished materials. The passage of the 1976 revision eliminated that distinction. As one analyst put it while discussing the "fundamental principles" of copyright, "the first of these is that literary property loses the protection of common law when it is placed under the protection of statutory law."[60] The statute itself made this explicit. Section 301 declares that "all legal or equitable rights [to works] within the general scope of copyright . . . , whether published or unpublished, are governed exclusively by this title." As the Senate report stated, "The preemption of rights under State law [that is, common-law rights] is complete with respect to any work coming within the scope of the bill, even though the scope of exclusive rights given . . . under the bill is narrower than the scope of the common law rights in the work might have been."[61] Unless other sections of the law provide for different treatment, it is hard to see how there could be any.

Those urging a restrictive application of fair use to unpublished materials have insisted that other sections of the 1976 law maintain the old distinction. They have pointed out that, in the reports accompanying the 1976 law, the House and Senate explained that the language in section 107 giving statutory recognition to the

[57] "News of the Week—Copyright Experts Explore New Perspectives and Old Conflicts," *Publisher's Weekly*, April 11, 1986, p. 18.

[58] Gary M. Peterson and Trudy H. Peterson, *Archives and Manuscripts: Law* (Chicago, 1985), 84–85, 88.

[59] "Copyright, Photocopying, and the Use of Manuscripts," session at the annual convention of the Society of American Archivists, held in Washington, D.C., September 2, 1984.

[60] Connor, "Literary Property in Archival Depositories," 144.

[61] Senate Report no. 473, 94th Cong., 1st sess., 114–15 (November 20, 1975).

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doctrine of fair use "is intended to restate the present judicial doctrine of fair use, not to change, narrow, or enlarge it in any way."[62] Because the inapplicability of fair use to unpublished materials was firmly accepted by the courts prior to the law's passage, the restrictionists have insisted that this language compels the conclusion that Congress intended to maintain the distinction. The Supreme Court agreed in its *Harper and Row* decision, citing the language of the reports in its opinion.[63] But the context of the language cited from the reports renders this restrictionist argument doubtful. After all, the language was not a comment on the preemption section (301) of the act, which explicitly eliminated the distinction between published and unpublished materials.[64] Rather, the language occurred in the context of a discussion of the criteria that judges had traditionally used to determine whether the use of published materials had been fair or not. In view of the clearly stated intention of Congress to eliminate common-law copyright in unpublished works, a more logical interpretation is that Congress simply wanted to confirm that, by incorporating the doctine of fair use into the statutory law, it did not intend to alter the criteria that judges had traditionally relied on to determine whether or not a particular use had been fair. Those criteria had formerly been applied only to uses of published works, because fair use had only applied to materials protected by statute. Now that unpublished materials are protected by statute as well, the logical conclusion is that the same criteria should be applied to determine whether or not an unauthorized use of unpublished materials has been fair.

This understanding of Congress's language is even more compelling in light of the specific reassurances that the House and Senate reports offered scholars who complained that a copyright term extending fifty years after the death of an author would inhibit scholarship. Scholars would retain fair use of materials of "scholarly value to historians, archivists, and specialists in a variety of fields," the reports explained, in a system that applied "equally to unpublished works, to works published during the life of an author and to works published posthumously."[65]

Finally, the argument that the congressional reports demonstrated the intention of Congress to maintain the common-law rules is inconsistent with the conclusion its advocates have derived from it. If section 107 were meant to "restate" the common-law rule on this subject and "not to change, narrow, or enlarge it in any way," then no unauthorized use whatsoever could be made of unpublished materials. There is no basis in common law for the conclusion that fair use may be made of unpublished materials in "extraordinary circumstances."

Those advocating fair use of unpublished materials only in extraordinary circumstances have also stressed the Senate report's comment that "the applicability of the fair-use doctrine to unpublished works is narrowly limited, since, although the work is unavailable, this is the result of a deliberate choice on the part

[62] Goroff, "Fair Use of Unpublished Works," 338, quoting House Report no. 1476, 94th Cong., 2d sess., 66 (1976); Volunteer Lawyers for the Arts, "Brief," 284, quoting the same words.
[63] *Harper and Row v. Nation Enterprises,* 105 S.Ct. 2227.
[64] Senate Report no. 473, pp. 114–15; House Report no. 1476, pp. 130–32.
[65] Senate Report no. 94, pp. 118–19; House Report no. 1476, pp. 134–36.

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of the copyright owner."[66] (This language appeared only in the Senate report, in one of the few places the House and Senate reports diverged. However, analysts have argued persuasively that the decision of the House to exclude the language did not indicate rejection of it.[67]) This language, restrictionists have argued, reinforces the implication that Congress intended to maintain the common-law distinction between unpublished and published materials in regard to fair use. The explicit language of the law, the Senate report's comments on the preemption section, and its reassurances to scholars render this conclusion questionable, too. Like the other language that restrictionists have relied on, this language appeared in the context of the Senate report's discussion of the criteria that ought to determine whether or not a use of a copyrighted work is fair. One of the most important criteria had traditionally been the commercial availability of the material. The more difficult it was to secure the material from commercial sources, the wider the latitude for fair use. It was in the course of discussing this particular criterion that the Senate report noted that the applicability of the fair-use doctrine to unpublished works should be "narrowly limited," because the unavailability is "the result of a deliberate choice on the part of the copyright owner." Given the context, the purport of the statement is clear. The Senate meant that commercial unavailability must not be as weighty a criterion for justifying unauthorized use of unpublished works as it is for published ones. The language of the report suggests that Congress's primary concern was to preserve an author's conscious decision not to publish, rather than to erect an arbitrary rule that use of unpublished materials can be fair only in extraordinary circumstances. The language suggests a further criterion for establishing fair use of unpublished materials: the degree to which the author desired to shield the work from publication and the degree to which its use would violate that desire for privacy. Thus, the fact that a known and respected author withheld a literary manuscript from publication more strongly suggests an intent to avoid its public exposure than the fact that correspondence from an ordinary citizen remained unpublished, and these circumstances should be taken into account when deciding whether a use was fair or an infringement.

The application of this less strained interpretation of the language of the Senate report would have provided an appropriate ground for protecting former President Ford's unpublished manuscript against the *Nation*'s unauthorized use of it. Plainly, in that case, both Ford and Harper and Row had made a conscious decision to withhold publication, and according to the Senate report, that should have been a key criterion in determining whether or not the unauthorized use was fair. Under the new law, the issue should have been the intent of the author rather than the status of the material. In fact, the language of the Supreme Court's *Harper and Row* opinion is consistent with this interpretation. Although the briefs sustaining Harper and Row used the term "extraordinary circumstances" to

[66] Senate Report no. 473, p. 64; see Goroff, "Fair Use of Unpublished Works," 337; Association of American Publishers, "Brief," 267; and *Harper and Row v. Nation Enterprises*, 105 S.Ct. 2227.
[67] Patry, *Fair Use Privilege in Copyright Law*, 445–47.

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characterize when a fair use might be made of unpublished materials, the Court refrained from doing so. Its opinion merely stated that the unpublished status of a manuscript was "a key . . . factor" in determining whether or not the use had been fair.[68] Thus, it may be possible that future decisions will distinguish the Supreme Court's position in *Harper and Row v. Nation* from more extreme restrictionist positions.

The *Harper and Row* decision did not involve photocopying. But since the general effect of the decision is to narrow the applicability of fair use to unpublished materials, *Harper and Row* may be taken to strengthen the position of the Copyright Office staff that the fair-use section (107) and most of the photocopying section (108) of the Copyright Revision Act are not applicable to unpublished materials. However, the argument of the Copyright Office to restrict the right to photocopy unpublished materials is also less than compelling.

The staff of the Copyright Office naturally sought support for its restrictive view from the language of the law, but its interpretations were questionable. For example, the report pointed out that subsection 108(e) requires libraries and archives to determine that a copy cannot be obtained at a fair price before making a photocopy for a researcher. Since unpublished materials are inherently unavailable from commercial sources, the staff argued, the provisions of section 108 authorizing photocopying for interlibrary loan or for researchers must apply only to published materials.[69] Nothing in the language compells such a conclusion. The law stated that a repository may photocopy materials for researchers unless they are available through commercial channels. From this, the Copyright Office staff concluded that only such materials as are available through commercial channels may be photocopied. This conclusion does not logically follow. It is the same as saying that if a law requires that all dogs brought to a park be leashed and muzzled, then only dogs are allowed in the park.

The history of the law's development indicates that the original concern was to prevent libraries from being so free with photocopies as to destroy the market for published works.[70] For that reason, Congress inserted a proviso to protect publishers. That insertion hardly suggests Congress intended to exclude unpublished materials from the photocopying law. If Congress had intended to do so, surely it could have said so directly. But where Congress did explicitly enumerate the types of works to which the photocopying section of the new law was inapplicable, it listed musical work, pictorial and graphic work, motion pictures, and other kinds of materials; it made no reference to unpublished literary works.[71] The report of the Copyright Office conceded as much when it recommended

[68] *Harper and Row v. Nation Enterprises*, 105 S.Ct. 2227.

[69] *Report of the Register of Copyrights*, 123–24.

[70] For the controversy over photoreproduction leading to the relevant sections of the 1976 revision, see Val D. Greenwood, "Fair Use and Photocopy," *ASCAP Copyright Law Symposium*, 24 (1980), 113–56; William Z. Nasri, *Crisis in Copyright* (N.Y., 1976). For examples of the deep feelings aroused, see U.S. Copyright Office, *Copyright Law Revision, Part 2: Discussion and Comments on the Report of the Register of Copyrights on the General Revision of the U.S. Copyright Law* (Washington, D.C., 1963), House Committee Print, 31–44, 256–57, 321–24.

[71] *Copyright*, U.S. Code, vol. 17, sec. 108(h) (1978).

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amending the law in such a way as to explicitly limit the photocopying provisions to published works.

In all the photocopying provisions of the 1976 revision, Congress referred to "libraries and archives." The staff of the Copyright Office perceived no significance in this, apparently considering the terms interchangeable. The staff itself used the term "library" to refer to both types of repository, they explained, "simply in the interest of brevity."[72] Yet, it is a basic canon of statutory interpretation that the legislature be presumed to have chosen the particular language of a statute in order to effect some purpose and that nothing be presumed superfluous. In common parlance, a library and an archive are not the same thing. A library is "a place set apart to contain books," according to the standard dictionary definition. An archive is "a place in which public records or historic documents are kept"; and, while a published book may be a "document" in the broadest sense, this definition is not what usually comes to mind. The dictionary gives as examples of documents "a manuscript, title-deed, coin, etc."[73] Congress had clear examples of both types of repository before it as it wrote the law: across the street from the Capitol stands the Library of Congress; halfway to the White House stands the National Archives. The first is filled with published books and periodicals, along with a manuscript collection that is of major importance but which makes up only a small portion of its holdings. The second is filled with unpublished materials, along with a small number of books and periodicals that make up only a small portion of its holdings. Although it is possible that Congress intended the photocopying provisions of the law to apply only to the small number of published materials in such an archive, it surely is not the most likely conclusion.[74]

To bolster the argument against fair-use copying of unpublished works (and, implicitly, any fair use of such works), the staff noted the comment in the Senate report that "applicability of the fair use doctrine to unpublished works is narrowly limited," since the work's unavailability "is the result of a deliberate choice on the part of the copyright owner."[75] Even if we put aside the accuracy of this assessment of the copyright owner's intentions, the language of the Senate report hardly sustains the contention of the Copyright Office report that the law provided for no fair use of unpublished work at all. On the contrary, the cited language explicitly recognized the applicability of the fair-use doctrine to unpublished work. It is difficult to see how "narrowly limited" can be construed to mean "none."[76] That the Copyright Office misinterpreted the Senate's intent becomes clear when

[72] *Report of the Register of Copyrights*, 67.

[73] *Oxford Universal Dictionary on Historical Principles* (3d edn.; Oxford, 1955).

[74] Congress may have referred to "archives" because film and photograph repositories are often called "archives." But, knowing from the example of the National Archives that the term would include document repositories as well, Congress would have specifically excluded unpublished materials from the coverage of the provision, had they intended it to cover only published materials. Moreover, Congress specifically excluded both motion pictures and photographs from the reproduction sections of the law; 17 U.S.C. 108(h). It is not likely that Congress would refer to "archives" in those sections and then prohibit their application to the only material it conceived to be in those archives. Plainly, "archives" means something more than film and photographic archives.

[75] *Report of the Register of Copyrights*, 106, citing Senate Report no. 473 at p. 64.

[76] See also Senate Report no. 473, p. 61 and following.

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one recalls the Senate's specific affirmation, earlier in its report, of Congress's commitment to providing access to materials of "scholarly value to historians, archivists, and specialists in a variety of fields."[77] In view of that commitment, it is not likely that when the Senate noted the "narrowly limited" applicability of fair use to unpublished works, it seriously meant to inhibit scholarly research by denying researchers the right to photocopy unpublished manuscripts.

Throughout the discussions by restrictionists of the applicablilty of the fair-use doctrine to unpublished materials, there runs a primary concern for the property rights and privacy of the copyright owner. As the justices' words in *Millar v. Taylor*, quoted above, make clear, those concerns lay at the heart of the common-law right of first publication.[78] But unpublished materials are now covered by the statutory copyright law, just as published works are, and the foundation of that law is not the protection of property or privacy. Rather, the Constitution explicitly states that the purpose for which Congress may secure patents and copyrights is "to promote the Progress of Science and useful Arts."[79] Since the 1930s, the courts have attended more and more to this expression of purpose in interpreting provisions of the national copyright law. As one court put it, the "reward guaranteed to the copyright holder is but an incident of this general objective, rather than an end in itself."[80] The most recent edition of one of the standard treatises on copyright law states flatly, "The actual congressional power is *to promote* the progress of science . . . rather than to secure rights."[81] The language of the 1976 copyright law must be interpreted with that ultimate purpose in mind. As the courts have said, reliance on the concept of fair use is the way we "occasionally subordinate the copyright holder's interest . . . to the greater public interest" that the copyright law is intended to secure.[82] Now that unpublished materials have been explicitly brought under statutory law, the same rule must apply to them.

This shift in emphasis should not be taken to mean that the claims of scholarship ought to extinguish the rights of copyright holders in their works. The presumption that all authors of unpublished materials consciously intended to avoid publication, which shaped both the restrictionists' discussion and that of the Senate, may seem naive to historians. But this presumption is understandable,

[77] *Ibid.*, 118–19. The discussion was in the context of published materials, because at issue was the change in term of copyright, and only published materials had been covered by the earlier law. The reference to archivists makes clear that the Senators and their staff had unpublished materials in mind as well. The Senate report also notes that the new system applies "equally to unpublished works, to works published during the life of an author, and to works published posthumously." *Ibid.*, 119.

[78] See text at note 13 for full quotation.

[79] U.S. Constitution, Art. I, sec. 8.

[80] *Berlin v. E.C. Publications*, 329 F. 2d 541 (2d Cir. 1964).

[81] Alan Latman, *The Copyright Law: Howell's Copyright Law Revised and the 1976 Act* (5th edn.; Washington, D.C., 1979), 15. See *Fox Film Corp. v. Doyal*, 286 U.S. 123, at 127 (1932); *U.S. v. Paramount Pictures*, 334 U.S. 131, at 158 (1932); *Berlin v. E.C. Publications*, 329 F. 2d 541 (2d Cir. 1964); *Rosemont Enterprises v. Random House*, 366 F. 2d 303 (2d Cir. 1966); *Meeropol v. Nizer*, 361 F. Supp. 1036 (S.D.N.Y. 1973), 560 F. 2d 1061 (2d Cir. 1967); and *Williams and Wilkins v. U.S.*, 487 F. 2d 1345 (Ct. Claims 1973) for the line of cases that have established this rule. However, the Supreme Court has drawn the authority of this reasoning into question in the *Harper and Row* case, stressing that the copyright provision of the Constitution was "designed to assure contributors to the store of knowledge a fair return for their labors." *Harper and Row v. Nation Enterprises*, 105 S.Ct. 2218 at 2223.

[82] *Rosemont Enterprises v. Random House*, 366 F. 2d 303, at 307 (2d Cir. 1966).

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considering that the discussion of copyright protection and fair use has traditionally concentrated on materials that have commercial value. It is the misfortune of scholarship that the personal correspondence of a minor political activist has been considered under the same rubric as Mark Twain's unpublished literary manuscripts. Historians must recognize that the category of unpublished works is far broader than those primary sources that they want to be able to quote and photocopy. As efforts are made to impose a restrictive interpretation of the applicability of the fair-use and photocopy sections of the 1976 law to unpublished works, scholars must disabuse Congress and the courts of the notion that every author of every kind of material has made a conscious decision whether or not to publish. Scholars in turn must recognize that there are many cases in which authors have indeed made a decision not to publish; and, in such cases, they and their heirs ought to retain the right to decide when that decision is to be reversed, a right of first publication, until fifty years after the author's death. In seeking to protect the public interest in the scholarly use of unpublished materials to forward knowledge, historians must not seek rules that would undermine the value of literary and artistic property to copyright owners or the right of privacy of their authors. Reasonable application to unpublished materials of the traditional criteria determining fair use, with the recognition that commercial unavailability cannot count for much, is the best guarantee that public interests and private interests will both be promoted.

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Reviews of Books

GENERAL

GWYNNE DYER. *War*. Homewood, Ill.: Dorsey. 1985. Pp. xii, 272. $13.00.

It may be a sign of unusual editorial sophistication and foresightedness that the *AHR* sent out Gwynne Dyer's *War* for review. The work is not a scholarly monograph nor even a textbook in the usual sense of the term. It grew out of a television series in which Dyer acted as host and commentator; in turn it became the text, which, together with an anthology of writings on war and a "faculty and administrator's guide," converted the series into a "college-level telecourse." But, although the book is not a work of original scholarship, it does indicate certain changes—perhaps not yet very great—in the way history is written and taught in this country. It is the kind of cultural artifact that historians will have to analyze when they begin to interpret American culture in the 1980s.

At least since the end of the Second World War, the study of war has not been held in high regard among most American historians. Some of the reasons are fairly obvious: anxiety over the development of nuclear weapons, horror at the human and social costs of war, disagreement with American military policies, especially in Vietnam—all have contributed to make the history of war suspect, or worse. Understandable as the distaste for the subject is, however, our discipline has suffered from the failure to study war with the same intensity and intelligence that have been devoted, for example, to popular culture or the social effects of industrialization. Recently this has changed. More colleges are offering courses in the history of war; the serious literature on war seems to be increasing; the AHA has even established a prize for the best historical work in the field.

One significant element in this rekindled interest is the wish to understand war as a general phenomenon. It is true that the literature is still surprisingly weak in monographic analyses of particular wars and specific episodes in the history of military thought and institutions. But the attraction of broad synthesizing treatments is unmistakable, stimulated, no doubt, by the general crisis of the nuclear age. In the eyes of many, war has changed from a valid instrument of state policy to a device for universal suicide; to be disarmed, war needs to be understood. Dyer's book is a product of this concern. Its contemporary orientation is very clear: over one-third of the text discusses war from prehistory to 1945; the rest addresses the prospects of nuclear war, theories of deterrence, attempts to discover ways of fighting "limited" nuclear wars (a particularly trenchant section), military professionalism, and the ease with which civilians in any society can be trained to kill and be killed in the service of the state. The final chapter, "Goodbye War," is, as its title suggests, a plea for replacing narrow traditional loyalties with an appeal to "a wider humanity." The whole work is suffused with a sense of urgency; its treatment of the evidence of past and present may be characterized as realistic in detail, visionary overall.

What is the function of history in such a work? An analysis of many thousands of years in a hundred pages is inevitably superficial, but in general Dyer employs history with reasonable accuracy to demonstrate the close interaction between war and political culture throughout history. That is a useful point to make. It is, however, preceded by a labored attempt to prove on the basis of our knowledge of Australian aborigines, South African bushmen, and certain American Indian tribes that prehistoric war was primarily a ritual rather than a serious contest in which the losing side might be severely weakened or even destroyed. The author needs to establish the ceremonial character of prehistoric war because he believes it lends credence to his thesis that men and women are not innately aggressive killers but that war is a result of civilization—military power "was the key invention of the first civilized men" (p. 11)—and therefore susceptible to abolition.

But the unserious nature of prehistoric war—even if it could be proved—is of doubtful value in arguing that war can be eliminated; Dyer, moreover, joins the argument with a strangely one-sided

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analysis of the decades since Hiroshima and Nagasaki. His discussion of the nuclear buildup and theories of nuclear war is clear and well-informed, but he has remarkably little to say about the many conventional wars that have been and are being fought in the nuclear age. That nuclear weapons have not made war impossible weakens the author's neat progression from primitive bloodletting to the destruction of the civilized world. But that conventional wars can still be fought is one of the most important facts of contemporary life, a fact that, in its dreadful and unstable way, is even reassuring.

So in the end Dyer's study is not very good history. It may, however, be important history—a reflection of broad anxieties that cause people other than professional historians to study and interpret the past and to do so not for history's own sake but to find a way through the present.

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ERNEST R. MAY, editor. *Knowing One's Enemies: Intelligence Assessment before the Two World Wars*. Princeton: Princeton University Press. 1984. Pp. xiii, 561. $29.50.

Much of the literature on the history of intelligence remains deservedly ephemeral. To the nonspecialist the field undoubtedly seems dominated by narrowly conceived memoirs, lightweight accounts that are short on analysis and context but long on drama, and scholarly works that show a dismaying appetite for organization charts and thinly disguised speculation.

A collection of essays, originally offered in 1980 as conference papers, is perhaps unlikely to upset the disposable quality of the literature. Ernest R. May's volume, however, does just that. The contributors set a new standard for both scholarship and interpretation. Collectively, they make a substantial contribution to our understanding of how intelligence miscalculations helped precipitate two world wars and affected their outcomes. The standard Second World War accounts have already begun to capitulate to the new findings based on Ultra and Magic. This volume suggests that the same process of reinterpretation will have to be applied to the prewar eras.

The starting point for any such reinterpretation is a concept of intelligence that reaches far beyond the spy-spymaster syndrome to accommodate the processes of information collection, analysis, and dissemination. Intelligence assessment is a complex and diffuse task, above all, an activity that is determined by the power structure and official mentality of the state. One of the most stimulating features of this volume is the degree of intellectual warfare waged between the contributors on the relative importance of bureaucratic politics and ideas in the making of intelligence assessments.

May leads off by ascribing a predominant influence to the power structure in shaping Russian, German, and British assessments of potential enemies before 1914. Autocracy in Russia and Germany dictated extreme centralization of intelligence and ultimate dependence on the judgment of the leader; the British cabinet-style democracy produced decentralization of the intelligence process and ultimate muddle. Most of May's contributors, however, rebel against this methodology and instead favor the importance of *mentalité* in shaping assessments. Norman Stone, discussing Austria-Hungary, openly refutes May's approach. The Habsburg monarchy, like most states before the war, possessed good intelligence and a workable assessment system but failed to put them to effective use because of a faulty vision of future war. Paul Kennedy finds a similar problem at the heart of British intelligence before 1914, particularly affecting the Royal Navy's vision of an epic encounter between the British and German battle fleets. Essays by William Fuller and John Gooch show that both the Russians and the Italians were prone to anxiety and insecurity, which, when joined to a belief that a future war would be short and inevitable, was an explosive combination. France, by contrast, was gripped by an unshakeable strategic optimism in 1914, supported by a preconceived view of how the German military would conduct a war in the West. The view was wrong, the optimism misplaced—all a species of "wishful thinking," as Christopher Andrew concludes. Jan Karl Tanenbaum explores this wishfulness in more detail and provides a convincing picture of how the French simply projected their own military maxims onto the Germans. Perhaps the most striking characterization of the role of *mentalité* is found in Holger Herwig's essay on imperial Germany. Political and military leaders of the Second Reich were prepared to risk war in 1914 because they fed all their intelligence through the mill of a *Götterdämmerung* mentality.

Further evidence of the power of preconceived ideas emerges in the discussion of the pre–World War II period. The "short-war illusion" returned to Europe in modified form in the 1930s. Donald Cameron Watt shows that British intelligence perceptions were influenced by a belief that the Third Reich could afford only a short, sharp war, owing to economic difficulties and an indifferent population. The result was a traumatic emphasis on the likelihood of a German aerial knockout blow—a strategy that the Luftwaffe was simply unable to attempt until its medium-range bombers and short-range fighters were parked on the French channel coast.

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French appeasement policy, like the British variant, rested on an exaggeration of German offensive military capability and a misunderstanding of how it could be used. Robert Young uses the concept of *mentalité* effectively to describe the overpowering impact that the historical lessons of the Great War had on a rigidly conformist-minded French officer class that bent all its energies to construct a set-piece defense. David Kahn, Michael Barnhart, and Peter Lowe all find cultural stereotypes and racialist preconceptions dominating American, Japanese, and British readings of the strategic situation in the Pacific. Most elaborate of all was the logic that led Stalin to believe that he could avoid a war with Hitler, or at least that he could depend on such a war unfolding according to the classic rules of ultimatum, frontier skirmish, and slowly developing main battle. John Erickson here provides a fascinating excursion through the Soviet literature.

Only when one comes to an examination of the intelligence systems of Nazi Germany and Fascist Italy does there appear any support for May's preferred approach. The intelligence worlds of both dictatorships, as Michael Geyer and Macgregor Knox show, were characterized by anarchical structures and a Social Darwinist struggle for survival. Knowledge was power; Führer and Duce alike had to control the major share. Geyer is particularly good at showing that intelligence truths were debased in the Nazi system to a mere commodity, to be traded for influence and the Führer's attention.

May provides a strong summary, setting aside the running debate on organization versus ideas to concentrate on the historical lessons of net assessment. Two stand out. The first is that, even in the brief period between 1914 and 1939, intelligence work grew enormously more complicated as bureaucratic expansion and changing weapons technology made themselves felt. The second lesson is that, in order to strive for realism, intelligence services must combat the distorting effect of preconceptions by an unremitting process of self-reflection. Knowing one's enemies proves to be dependent on knowing oneself.

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FRANKLIN L. FORD. *Political Murder: From Tyrannicide to Terrorism.* Cambridge: Harvard University Press. 1985. Pp. xii, 440. $29.50.

Franklin L. Ford is well known for his solid studies on French and European history during the *ancien régime* and the revolutionary and Napoleonic era. His latest book encompasses a vastly wider scope, ranging from the Old Testament to the morning headlines. One-fourth of this very readable work concerns salient instances of "political murder" in the ancient world down through the expansion of Islam. The focus of the next quarter extends from the medieval period through the age of Napoleon. The latter half deals with the nineteenth and twentieth centuries; one-fourth of the book's pages are devoted to events since World War II.

The term "political murder" can be and is applied to judicial and administrative decisions and to terrorist action carried out to achieve political leverage, but the emphasis here is on assassination. An attempt is made to identify those few periods in the West when political murder was relatively infrequent and to single out the characteristics they may have shared. Ford reports that "high assassination rates have not generally accompanied extremes of repression and perceived social injustice" nor coincided with major wars but they are increased in "post-revolutionary, post-colonial, and postwar situations" (pp. 382–83). Religious or ideological zeal has, of course, animated countless assailants; individual actors have been more successful than cabals in the history of assassination attempts.

So broad a survey mandated a rigorous selection of examples. All in all, the proportions of allotted space seem sufficiently balanced. Inevitably, as with literary anthologies, most readers, though acknowledging the discovery of material previously unknown, will wonder why some items were not included. The unexpected, at least to this reader, included interesting material on Abyssinia from the twelfth through the fifteenth century. Here, as elsewhere, Ford communicates his zest for the subject and its significance.

Among the surprises, if not disappointments, is the relatively brief section on the revolutionary and Napoleonic age—a total of six pages, which is roughly the space given to the Orsini assassination attempt against Napoleon III. Even allowing for the author's effort "to avoid laboring analogies" (pp. 4–5), a bit more on the terror—including the Jacobin network that foreshadowed the one-party state—might have been useful. Napoleon's regime is equally apposite; the execution of Enghien is not mentioned.

Ford deftly encapsulates viewpoints that advance the discussion, as in the sections on Thomas Aquinas and Niccolò Machiavelli. He also has an instinct for the striking quotation. Yet, although Saint Paul may be eminently quotable, he is inadvertently credited with one line too many: "Paul's famous admonition to 'render unto Caesar what is Caesar's' was no doubt a prudent rule for survival in the period before Emperor Constantine's adoption of Christianity, though even then the Apostle reserved 'what is God's'" (p. 120).

Apropos the early church, Ford might have noted (and expatiated on) the qualifications added to the

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commandment "Thou shalt not kill," particularly in reference to heretics. When he later treats the execution of Servetus in 1553, he claims it "offered startling proof of the lengths to which a Calvinist regime in power would go in combating what it considered heresy" (pp. 152–53). If anything, the execution was in the grand tradition; a similar fate would have befallen Servetus in countless other communities in Christian Europe.

The array of multilingual sources includes effective use of material drawn from Jesuit archives in Rome. Extensive bibliographical references in the notes are especially useful, since contrasting interpretations are frequently cited; the case of Julius and Ethel Rosenberg is a fair example of this approach.

Ford is to be commended for an ambitious and much-needed perspective on contemporary events. One of his abiding concerns was to determine how often assassinations have, in fact, been able to effect the political results intended by their perpetrators. Thus far, it has happened very seldom. Yet recent terrorist-suicidal attacks against American and French military camps in Lebanon doubtless achieved more (and at less cost to the terrorists) than would have been possible had they engaged in conventional warfare. Indeed, the very frequency of terrorist strikes has almost made them appear "conventional." On this timeliest of subjects, much more will be added to the dossier before the century ends.

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J. LEE READY. *Forgotten Allies: The Military Contribution of the Colonies, Exiled Governments, and Lesser Powers to the Allied Victory in World War II.* Volume 1, *The European Theater*; volume 2, *The Asian Theater.* Jefferson, N.C.: McFarland. 1985. Pp. xviii, 470; x, 219. $39.95 the set.

This curious work by a World War II buff is substantial and valuable because of its myriad details, for which reason it ought to be included among reference books of World War II. J. Lee Ready's purpose is to record and to draw attention to the military contributions achieved by the minor Allies during that war. This includes the armed forces of the exiled governments, of the British Commonwealth, of the Free French, and of many others, ranging from a Brazilian division to Filipino guerrillas. The larger first volume deals with the fighting in Europe, Russia, the Mediterranean, and Africa; the second examines the Pacific and Asian theaters of war. Many of the details are new and not easily available elsewhere. The author seems to have had a special interest in relatively minor operations—in the Aegean and in Abyssinia, for example—and in the records of Polish and Free French forces serving with, or alongside, the British. He must have devoted extensive time and commendable effort in searching for the names of outstanding commanders, captains, and pilots; of identifiable units; of the numerical records of their achievements; and of their casualties. Yet the two volumes have only incomplete bibliographies and practically no references. The few footnotes are throwaway remarks, reflecting at times the summary wit of the author.

This somewhat cavalier absence of "scholarly equipment" is, alas, not the only shortcoming of the work. The author is obviously deeply interested in—and, I should add, very knowledgeable of—military history. Consequently, many of the chapters diverge from his main purpose and consist of detailed, opinionated (but always readable) descriptions of the main military operations, plans of campaigns, and achievements and shortcomings of their Allied commanders. Of course, the military contributions of the minor Allies cannot be described apart from the campaigns of the major Allies, but one has the impression that here are matters for another book that the author has somehow telescoped into these two volumes. Last but not least, his extensive and often judicious knowledge of the military history of World War II is not equaled by his knowledge of the political history. His account of the events and the people who were involved in the national civil wars between pro-German and anti-German, pro-Japanese and anti-Japanese, governments and forces is at times marred by mistakes and even misspellings of names. Yet, in sum, these two volumes are not only significant but also important contributions and correctives to our vast accumulation of historical literature on World War II.

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MARK GIROUARD. *Cities and People: A Social and Architectural History.* New Haven: Yale University Press. 1985. Pp. vii, 397. $29.95.

One of the attractions of studying urban history is the fascinating variety of cities themselves, yet the spatial and architectural features that distinguish one city from another rarely receive attention from scholars. Mark Girouard has tried to integrate the distinctive features of individual cities with aspects of urban development familiar to economic and social historians. His book begins with medieval Constantinople; proceeds to the Italian, Flemish, and German cities of the Middle Ages; and pauses in Renaissance Rome before leaving the Mediterranean for Antwerp, Amsterdam, Paris, London, the provincial cities of England and France in the eighteenth century, colonial cities in India and America,

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and nineteenth-century industrial cities. The omission of cities in the Iberian peninsula, southern Italy, and Scandinavia diminishes the value of the book by narrowing its range. The criteria for selection are not stated in the text, but Girouard seems most interested in cities that have generated innovative architectural and spatial forms during a phase of growth. Even within this category he is selective: with the exceptions of Paris and London, cities that have had several periods of growth receive extended treatment only once; sixteenth-century Antwerp, for example, is discussed, but late nineteenth-century Antwerp is not.

Although Girouard gives prominence to economic factors, he has no systematic interest in the reasons why some cities grow and others do not. He does not consider what happens to once-prosperous cities in decline, even though decline is as relevant as growth to an understanding of environmental change. Nor does he consider the type of city that never generates significant growth. As Jane Jacobs observes in *Cities and the Wealth of Nations* (1984), people generate urban growth through social and economic innovations, which they integrate into everyday life by modifying the urban environment. Part of the book's strength is that Girouard gives ample evidence of this phenomenon; part of the book's weakness is that he fails to use his evidence to draw conclusions about larger patterns in urban development.

Girouard is best when discussing the social uses of space and the creation of new uses of space. Girouard implies that great cities before 1800 offered creative people more opportunities to provide attractive buildings and spaces for everyday life. But we learn little about who builders in cities were, how designs were elaborated between architects and clients, and what factors affected the transfer of ideas about building from one city or period to another. Considering that Girouard expresses "sympathies and admiration" for those who supplied cities "with drainage, services, transport, hospitals, schools and open spaces" (p. vii), the paucity of material on large-scale modern engineering structures and systems is surprising.

Girouard's sources are period guidebooks, memoirs, and modern secondary studies. In such a book the notes are obviously not meant to guide the reader through the library. Pulling out details from the works of others, Girouard has something fresh to say, as in numerous passages concerning trees in the urban environment, a discussion of the metaphor of the city as Babylon in the nineteenth century, and a comparison of early skyscrapers in New York and Chicago. But he also has passages that are simply misinformed, as when he states that "from the sixteenth century onwards the opportunities for designing a new town of any size in Europe were few and far between" (p. 220).

The book is richly illustrated, but the sources of some illustrations are given in the caption, whereas the sources of others are listed only at the end of the notes. Considering that the illustrations range from medieval to modern materials, the absence of comment on different conventions for depicting urban scenes is unfortunate. Girouard should have added a map to show all the places mentioned in the text.

Girouard's intention was "to start with the functions which have drawn people to cities, and to work outwards from them to the spaces and buildings which grew up to cater to them" (p. v). He does not wholly succeed at this important and difficult task, perhaps because his method, which is essentially descriptive, deflects his interest away from some of the interpretive and analytic issues that are suggested by the very nature of his theme.

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JON ELSTER. *Making Sense of Marx.* (Studies in Marxism and Social Theory.) New York: Cambridge University Press or Editions de la Maison des Sciences de l'Homme, Paris. 1985. Pp. xv, 556. Cloth $49.50, paper $15.95.

Jon Elster's book is a long and critical examination of the many facets of Marx's social theorizing. The sheer ambition of Elster's project, the polymathic learning and command of Marx's sprawling corpus that he brings to bear, and the high level of analytic skill and argumentative rigor displayed by this book put it virtually in a class by itself, justifying the arrogance implicit in the title. The author presupposes a reader who knows Marx and who is interested in the most detailed dissection and assessment of his thinking.

Elster devotes major chapters to Marx's philosophical anthropology, economics, and theory of history and to Marx's views on exploitation, class, politics, and ideology. In almost every area Elster manages to break new ground by considering previously unexplored issues or by offering fresh distinctions and insights. Precious little in Marx, however, withstands the scrutiny of a modern social scientist committed, as Elster is, to the austere canons of methodological individualism and explanation based on the rational choices available to a person. Although Elster credits Marx with some important, methodologically sound insights, these were invariably interlaced with dubious functional claims and dialectical deductions. Elster sees them as holdovers from Hegel and is tireless in his efforts to ferret out such pseudo-explanations in Marx.

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Elster largely demolishes what Marx took to be his major theoretical accomplishments. His critique finds nothing worth salvaging in Marx's economic theories, and Marx's theory of history fares little better. The author's discussion of historical materialism follows in the wake of G. A. Cohen's well-received defense of a traditional interpretation of that theory. Elster employs great sophistication in trying to unravel Cohen's tidy reconstruction and to expose Marx's ideas as a shambles.

The final two hundred pages are the best. Although critical of Marx's ideas on class, politics, and ideology, Elster apparently finds Marx's reflections on these matters more fertile. Elster elaborates a new definition of class and provides an intelligent commentary on class consciousness, collective action, and coalitions. He develops with care Marx's "abdication theory of the state" (that the bourgeoisie perceives its interest to be best served by leaving the management of the state to some other group or class). And he goes a long way toward rescuing a Marxist analysis of ideology from its "deserved ill-repute" by drawing on recent cognitive psychology to help furnish the needed "microfoundations" for such a theory. He also discusses succinctly and well Marx's own ruminations on ideology in the history of economic thought.

The cumulative effect of Elster's investigation is so thorough and seemingly devastating a critique of Marx that one wishes Elster had endeavored to convey the initial attraction, plausibility, and excitement that Marx's writings have had for several generations of readers. His conclusion does praise Marx for certain accomplishments, and on the last page Elster identifies himself as still, in a sense, a Marxist. But, compared to what has gone before, the praise seems faint and Elster's identification idiosyncratic. In any case, no serious student of Marx can ignore Elster.

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JUKKA PAASTELA. *Marx's and Engels' Concepts of the Parties and Political Organizations of the Working Class.* (Acta Universitatis Tamperensis, series A, number 199.) Tampere, Finland: University of Tampere. 1985. Pp. v, 341.

Jukka Paastela, a young Finnish scholar and socialist activist, presents Marx's and Engels's concepts of the party and political organization in roughly chronological order. Opening with three chapters of introduction, he follows with five increasingly long chapters dealing with the concepts of Marx and Engels up to 1847, the Communist League years (1847–52), the years from the end of the league until the formation of the First International (1852–64), the period of the First International (1864–72), and, finally, the years of the emergence of socialist workers' parties in Western Europe until the deaths of the two men (1872–95).

Paastela concludes that the concepts of Marx and Engels varied greatly depending on their interpretation of the state of development of the proletariat and the political and economic environment of specific countries and of Europe in general. Usually they favored a separate workers' political organization that would cooperate with bourgeois forces until the bourgeoisie assumed power; then the workers' party was to pursue a distinct and radical line leading to the inevitable conquest of political power by the proletariat itself. Marx and Engels, Paastela asserts, always opposed Blanquist-style conspiratorial organization and putschism, never held a concept of a vanguard party except when the consciousness of the proletariat and economic and political conditions were most primitive, and foresaw, vaguely, a postrevolutionary political form in which parties played no role but in which "all people were to enter [into] political decision making, obviously in relatively little political units" (p. 297).

This book fits into the increasingly popular school that finds strong democratic and antiauthoritarian impulses to predominate in the theories of Marx and Engels; Richard N. Hunt's two-volume study, *The Political Ideas of Marx and Engels* (1974 and 1984), the second volume of which Paastela reports he was unable to consult (p. 12 n. 17), is the outstanding example of this trend. Paastela's work is less detailed than Hunt's but supplements it in some areas.

An excellent command of secondary works, especially in pointing out where his interpretation differs from previous ones, lends conviction to Paastela's arguments. He has also made good use of the primary sources—Marx's and Engels's own works and the correspondence between these two men and the major figures of political socialism during the mid- to late nineteenth century. To those who have kept up with developments in Western Marx studies, his conclusions are not surprising, but they are nonetheless welcome as further substantiation of an emerging consensus on the true nature of Marx's and Engels's political views.

Unfortunately, Paastela's work is irritatingly flawed by inadequate English and, above all, poor proofreading and editing. That he is not a native speaker of English excuses much of the awkward construction and language; in fact, the book is a heroic achievement, as will be recognized by all who have tried to compose major works in languages other than their own. But the numerous typographical errors and misspellings and the sloppy editing make the awkward English even more irritating than it should be. Some of these errors result in

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unintended humor, as when a translation of the German word *Pest* (plague) is printed as plaque (p. 101), a much less serious disorder, and when Engels recognizes the *kleindeutsch* solution to German unification as "*faith accompli*" (p. 181), a charming slip of the printer's press or computer. But the many hundreds of similar and more egregious errors in the text and notes weaken the impact of Paastela's argument.

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JAMES SCHMIDT. *Maurice Merleau-Ponty: Between Phenomenology and Structuralism.* (Theoretical Traditions in the Social Sciences.) New York: St. Martin's. 1985. Pp. viii, 214. Cloth $30.00, paper $10.95.

To most historians, Maurice Merleau-Ponty (1908–61) is best known as the author of *Humanism and Terror* (1947). This is unfortunate, for (responding to Arthur Koestler's *Darkness at Noon*) the work advances a philosophically embarrassing and factually ill-informed *post hoc* justification for the Moscow purge trials of the 1930s. Merleau-Ponty is more worthy of attention than a reading of *Humanism and Terror* leads one to believe. One of the most interesting minds of his generation, he was part of a continuing intellectual movement that challenges the Cartesian foundations and Kantian principles of the humanities, social sciences, and even natural sciences.

James Schmidt's chief merit is the clarity with which he chronicles Merleau-Ponty's career, showing how his ideas evolved under the impulse of reflection and events. This is no mean accomplishment, for Merleau-Ponty's intellectual turns are sometimes bewildering. He began as an exponent of phenomenology, much influenced by Edmund Husserl's call for the description of human existence as it is actually lived. He ended up on the brink of structuralism, having been the first to try to put Ferdinand de Saussure's *Course in General Linguistics* (1916) to philosophic use. But he remained on the brink, and, when "the structuralist movement" burst onto the scene in the 1960s, he seemed outmoded. Approaching his work from a reading of his more explosive successors, Michel Foucault and Jacques Derrida, one is struck by the seeming hints (particularly in his late writings) of philosophical and literary themes that they were to develop. Schmidt helps us reinsert these themes into a context that is not merely one of anticipation.

Merleau-Ponty points the way, Schmidt contends, toward a reconciliation of tendencies in the human sciences that currently stand opposed to each other. As Schmidt notes, Merleau-Ponty was attracted to phenomenology in part because it seemed open to contributions from the empirical sciences. For his first two books he mastered a great deal of empirical research in behavioral and Gestalt psychology; later he dealt seriously with work in developmental psychology, linguistics, sociology, and anthropology. In short, he tamed the "excessively subjectivist" stance of phenomenology with "a knowledge of the opacity and density of the world of structures." Schmidt suggests that contemporary social theorists face the opposite problem, that of allowing room for subjectivity (pp. 166–67). For too long they have "partied at Durkheim's house," sharing Durkheim's disdain for individual motivation.

It is worth adding that this is a problem in many parts of the social sciences and not just in the relatively restricted domain of social theory. For example, an economist who suggests that one might study the behavior of businessmen by interviewing actual specimens is liable to find himself hooted out of the seminar room. Such attitudes have deep and largely unnoticed philosophical roots. We probably do not need Merleau-Ponty to tell us that something is wrong here; all we need is to learn to think for ourselves. But this is more easily said than done.

ALLAN MEGILL
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JED Z. BUCHWALD. *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century.* Chicago: University of Chicago Press. 1985. Pp. xiii, 339. $70.00.

This is a book for specialists. Since 1979, Jed Z. Buchwald has been publishing papers on nineteenth-century electromagnetic theory, particularly as it was developed by the followers of James Clerk Maxwell. These essays have now been revised and collected as a book; supplemented by new sections on the European response to Maxwellian theory, they form one of the first detailed examinations of how electromagnetic theory developed and changed in the 1880s and 1890s. The subject is intrinsically demanding, and Buchwald treats it with uncompromising rigor; his book is filled with equations and concludes with fifty-two pages of mathematical appendixes. As a result, it can hardly be expected to find a wide readership. But it will be carefully read by a few, and that is perhaps more important.

The book has two main aims: to explicate the essentials of the Maxwellian tradition and to trace the transition to electron theory in the 1890s. In his longest chapter, "Maxwellian Basics," Buchwald exhibits the underlying coherence of the Maxwellian system and shows how it differs from modern electromagnetic theory. In the process he clears up some persistent misunderstandings about how the Maxwellians viewed charge and current and points

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out the difficulties inherent in their conception of conduction current as the frictional decay of dielectric displacement. Indeed, most of the book is devoted to tracing how and why this conception was eventually abandoned in favor of treating current as a flow of electrons. Here Buchwald takes an unusual tack: instead of focusing on H. A. Lorentz and the problem of moving bodies or on J. J. Thomson and cathode rays, he concentrates on the seemingly peripheral subject of magneto-optics and shows that it played a greater role in the shift away from strict Maxwellianism than one might expect.

Buchwald's best chapters are those on the work of Edwin Hall and Joseph Larmor. By drawing on Hall's notebooks and Larmor's correspondence with G. F. FitzGerald, Buchwald is able to show how each man's ideas emerged and took shape; we see not just the finished product displayed in their published papers but also the trail of halting steps by which Hall and Larmor arrived at their new conceptions. It is especially revealing to see FitzGerald virtually dragging Larmor into including electrons in his ether theory; Larmor's published papers barely hint at FitzGerald's contribution.

Although Buchwald acknowledges that the various schools of thought in electromagnetic theory were partly shaped by social and institutional factors, he deliberately leaves these aside in order to concentrate on explicating the ideas themselves. As a result, some important questions are left unexplored. How did the Maxwellians function as a group? What role did they play in the larger society? How did ideas circulate within the British and Continental scientific communities, and how did they pass from one to the other? The abstract issues Buchwald addresses are in fact closely bound up with questions such as these, and his account would be both fuller and more widely interesting if it included some discussion of them. But this hardly detracts from the strength of the book as it stands; as an examination of the conceptual and mathematical foundations of late nineteenth-century electromagnetic theory, it is unlikely to be superseded.

BRUCE J. HUNT
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GLADYS L. HOBBY. *Penicillin: Meeting the Challenge*. New Haven: Yale University Press. 1985. Pp. xxii, 319. $32.50.

The historical literature on penicillin has tended to focus on the controversy surrounding its discovery—first by Alexander Fleming in 1928, again by Howard Florey and Ernst Chain in 1939–41—while neglecting the development of the antibiotic from a bacteriological curiosity to doctors' most potent weapon against infection. A massive wartime effort involving collaboration between British and American researchers in government, universities, and drug companies—medicine's equivalent of the Manhatten Project—was necessary to learn the properties of the mould, the range of its therapeutic applications, and, above all, how to produce it in large quantities. Gladys L. Hobby, a microbiologist who was involved in the American work on penicillin from the beginning, first at Columbia University and then at Pfizer, struggles to tell the story of the developmental phase in *Penicillin: Meeting the Challenge*. She does not succeed very well.

The book is based on the published research record, a limited amount of archival work, interviews with some of the other pioneers in penicillin research, and Hobby's personal recollections. This study contains a more or less complete record of who made what step forward when, but it is badly flawed by the author's apparent unawareness of the kinds of questions historians might ask of her material (or the diligence with which historians of the subject will almost certainly ferret out much more archival data). Hobby makes no coherent exploration of the intricate personal and material constraints on British research, for example, that caused Florey and Chain to bring their work to the United States. She only partly explores the interplay of technical problems and commercial considerations impeding American pharmaceutical companies' work on penicillin (it seems that drug companies were hesitant to invest in large-scale fermentation equipment because of their fear that cheap synthesization techniques were just around the corner). The author also has difficulty deciding whether any of the problems the researchers encountered should weigh against the magnificent achievement she is chronicling. As teller of a Whiggish tale of glorious therapeutic revolution, she is ultimately more interested in the grandeur of the results than the subtleties of the process.

Hobby would have been well advised to have enlisted the help of a good historian of science and several researchers in her endeavor to write a definitive history of the international penicillin projects. Or she could have lowered her sights and written an intimate personal account of the people and events she knew at first-hand. Her book fulfills neither task adequately and will be of limited value to future historians. At best, it supplements the earlier work of Ronald Hare and Gwyn Macfarlane. In a sadly defensive epilogue Hobby says too much too openly about one of the book's most distressing weaknesses: "Those who worked with penicillin during its early developmental period will all agree, I feel sure, that it was the most exciting and rewarding period of their professional lives. Yet I doubt that the excitement, pleasure, even pride that we felt—as each step of the process was solved and each report of lives

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saved by the drug reached us—is readily apparent in the account" (p. 246). It is a dry and limited addition to the historical record, to be added to the shelves of those libraries required to stock all books on the subject.

MICHAEL BLISS
University of Toronto

GIANNI GRANZOTTO. *Christopher Columbus*. Translated by STEPHEN SARTARELLI. Garden City, N.Y.: Doubleday. 1985. Pp. 300. $18.95.

Gianni Granzotto's book, striking a pose of learned biography, works step by step through the navigator's life. At the outset of many chapters the author discusses scholarly opinions, accepting some and rejecting others. He then proceeds to write another chapter in a Columbus novel. The book supplies no notes, and we are left to wonder why Granzotto says, for instance, that after Columbus arrived in Lisbon (ca. 1476) "for all intents and purposes, he did not leave the city for many years" (p. 32), or, of the fleet that contained the *Niña* and the *Pinta*, that "in terms of nautical quality it was downright second rate" (p. 103), or that the death of Martín Alonso Pinzón wrung the heart of Columbus (p. 188). These are only three among scores of puzzling assertions.

Although Granzotto has dipped into many secondary works—largely unnamed—he expresses disdain (p. 286) for Columbus studies generally, except for those of Samuel Eliot Morison, Salvador de Madariaga, and Paolo Emilio Taviani, and claims to base his own account on primary sources. The result is confused and confusing.

Should we just ignore such a book? If no important and scholarly books were awaiting translation into English, perhaps we should. What hurts is that this book has been translated and published whereas many important works have been passed over. For example, Antonio Ballesteros Beretta's *Cristóbal Colón y el descubrimiento de América* (1945) outdoes either version of *Admiral of the Ocean Sea* (1942) as straightforward, thorough biography. Juan Manzano Manzano, despite his cranky ideas about the discovery, has contributed indispensable information about the sequence of Columbus's activities in Spain in his *Cristóbal Colón: Siete años decisivos de su vida* (1964). And Jacques Heers opened up a whole new range of analytical biography in his *Christophe Colomb* (1981). Not one of these important, detailed books has seen an English translation, and Taviani's endlessly useful *Cristoforo Colombo: La genesi della grande scoperta* (1974) had to wait for Taviani himself to contrive a translation with the aid of William Weaver. Yet Granzotto's book was translated and published. Should this tell us anything more than that Doubleday expected big sales quickly? What questions should we ask?

We do not have to ask whether punitive taxes on warehouse stocks frequently prevent good books from being published if they do not promise large sales. We certainly should ask whether, with our national deficiency in languages, publishers simply do not know what is available overseas. Is it this same national deficiency that has dried up dissertations on Columbus and left us without Columbists to agitate for translation and publication of needed scholarly works? Some would blame this drying up on the exhaustion of primary materials, and perhaps no more documents will be found to work on. Even so, Heers's compendious analysis of primary documents and the current disenchantment with the editing of the *Diario* show that much work remains to be done on the primary documents.

American scholarship on materials in languages other than English suffers from serious deficiencies. Can we find the causes and lobby some of them out of existence?

FOSTER PROVOST
Duquesne University and
John Carter Brown Library

ANCIENT

ROBERT GARLAND. *The Greek Way of Death*. Ithaca: Cornell University Press. 1985. Pp. xvi, 192. $22.50.

This slender volume contains a rich and remarkably complete collection of the abundant but scattered literary, artistic, and archaeological evidence on death in the ancient world as well as extensive bibliography on the subject. Robert Garland conceives of death as a process, a rite of passage, a mutual but changing relationship between the deceased and his survivors. The steps of this process are, to use his chapter titles, dying, the funeral, between worlds, life in Hades, and visiting the tomb. For each he describes the beliefs, terminology, customs, rituals, offerings, artifacts, laws, deities (such as Hades and Hermes), and the various major and minor mythological and literary figures (such as Cerberus and Death himself). He discusses, apart from the process of death, the power and status of the dead and the various types of special dead, such as murder victims and suicides. The only omission, but one that limits the reader's understanding of the subject, is a sustained treatment of the cults of heroes such as Theseus and Heracles.

The model of rites of passage—separation, liminality, and reintegration—clearly suits the Greek mourners who, under the guise of pollution, were banned by a relative's death from various social

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activities and then, after rituals and an interval of time, were fully reintegrated into society. But Garland also argues that the Greek ritual of death served as a rite of passage for the dead person himself. Explaining separation is obviously no problem for the author, but in his discussion of the deceased's liminality and integration into the society of the dead Garland must depend on overly simplistic interpretations of highly complex literary and archaeological evidence. In this regard some will wish Garland had better heeded his own foreword (pp. ix–xiii). Despite his promise to focus on the geometric to classical period, Attica, and the "ordinary Greek," the author ranges through the millennia, the whole Greco-Roman world, and all types of evidence. Such expansiveness, though laudable, requires constant attention to the possibility of anachronism and to the vastly differing quality of heterogeneous sources. But in the text, unlike in the foreword, Garland usually takes for granted, first, that ordinary Greeks believed death to be as their poets described it and, second, that rituals meant to Greeks what they appear to mean to us. A honey-cake was put in the tomb for the dead to eat and a cup for him to drink from. Such ideas may have generated the ritual millennia earlier, but there is not a shred of evidence that classical Greeks believed any such thing. Such ahistorical and rationalistic interpretation of ritual bedevils the anthropology of the ancient world. The first assumption is no less misleading. Two centuries of scholarship have taught us how much practiced religion differed, in the whole and in detail, from the mythology of Homer and the dramatists, but on page after page Garland describes, mostly without caution, practices and beliefs solely from these poetic sources.

Despite reservations about *Quellenforschung*, I find the book a most useful collection of evidence, sensibly organized (no small feat) and lucidly presented. It is spiced with some anthropological theory, though happily not with its jargon. Modern theories seem selected because they accord with the evidence, not vice versa. To readers willing themselves to apply the principles of the foreword with somewhat more rigor than the author, *The Greek Way of Death* is a valuable source on the Greeks and on the always-lively subject of death.

JON D. MIKALSON
University of Virginia

O. A. W. DILKE. *Greek and Roman Maps*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1985. Pp. 224. $25.00.

Although primitive maps were in use among the city-states of Mesopotamia as early as the third millennium B.C., scientific cartography developed only with the coming of the Greeks. It began in the sixth century B.C. with Anaximander's pioneer attempt at a map of the world. By the third century B.C., Eratosthenes had produced a world map; drawn to scale and marked off with parallels and meridians, it pictured the land from Spain to India as roughly trapezoidal in shape and surrounded by water. In the second century A.D., Claudius Ptolemy mapped a greater area—by this time men had sailed further into the Atlantic and across the Bay of Bengal—but he introduced those grievous errors that were to plague geographers and sailors for over a millennium. The Romans seem to have contributed that most useful kind of map, the road map (such as the precursor of the Peutinger Table, which shows the major roads from Britain to India on a handy scroll twenty-one feet long and a mere one foot wide). All this O. A. W. Dilke tells, along with much more that is peripherally related to maps: he explains at length the Roman technique of surveying land, reviews the various land and sea itineraries that have survived, and discusses the kinds of plans the ancients drew, from plans of cities to "blueprints" of buildings.

He performs a useful service in bringing together this material, some of which has been available only in scattered scholarly journals. But his organization of it leaves much to be desired. He treats some of the material chronologically and some topically, using at times topics that are curiously narrow (for example, "Roman State Plans" [chap. 7] and "Maps in Art Form" [chap. 10]). This results in a narrative that moves in fits and starts. Thus, in chapter 2, "Evidence from Early Greece," he carries the story of maps down through Eratosthenes; he then backtracks in chapter 4 to take up geographical writers from Homer to Ptolemy. He breaks up the post-Ptolemaic period into two chapters, "The Development of Ptolemaic Maps" and "From Antiquity to the Renaissance," which results in inevitable repetition and a blurring of the line of development. Moreover, Dilke is unsure for whom he is writing. He provides elementary explanations of the sort to be expected in a work forming part of a series intended "for the general reader interested in classical antiquity," but, at the same time, he cites Greek words in Greek characters, leaves Latin words and phrases untranslated, drops names such as Cleomedes and Adamnan with no explication, and all too often burdens the text with scholarly wrangling over minor matters that should have been relegated to the notes or simply omitted (he even takes time to disapprove a suggestion put forth in an unpublished master's thesis!). The plates are of excellent quality but are only sporadically cross-referenced to the text. The documentation is adequate though not exhaustive. In short, this is by and large a book for

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specialists, but one that even they will find troublesome to use.

LIONEL CASSON
New York University

ELIZABETH RAWSON. *Intellectual Life in the Late Roman Republic*. Baltimore: Johns Hopkins University Press. 1985. Pp. ix, 355. $29.50.

Despite substantial revision or repudiation by the painstaking specialized scholarship of this century, traditional reductionist stereotypes of Greek and Roman cultures endure. The cliché of Greek rationality and creativity has spawned the ultimate caricature that "the Greeks had the brains, the Romans had the drains." The conventional stereotype of the Romans—that they were quintessential realists distrustful of novelty, parasitic assimilators, and superficial amateurs in intellectual pursuits—is commonplace.

Elizabeth Rawson's stunning new book is an illuminating corrective of this received Roman intellectual dilettantism. Focusing on the last few decades of the Roman Republic, Rawson spread before us the bustling bicultural intellectual life of Rome in the 50s and 40s, delineated with her magisterial control of ancient sources and modern specialized studies. With due attention to the social and political context of that turbulent period, Rawson marshals almost encyclopedic data on numerous disciplines and intellectual practitioners of the time. Judicious syntheses flow in and out of the expositions and are coordinated in a masterly summary conclusion.

The bearers of this cultural efflorescence were both Greeks and Romans. Rawson documents the brain drain from the Greek East to Rome (where the action and wealth were), the methodical importation of Hellenistic learning, and the active involvement of many among the Roman elites who withdrew from the political arena into private life and compensatory intellectual pursuits as a haven from the mounting general crisis and decay of the Republic. They took to Greek learning with zeal of converts and, in collaboration with Greek intellectuals, produced this first great intellectual flowering of Rome, which, as Rawson reminds us, was not one of the grand creative periods in world intellectual history.

Most of those who tilled this fertile ground were not figures of the first rank, except for Cicero and the polymath M. Terentius Varro. Others most frequently cited by Rawson are, among the Romans, Nigidius Figulus and T. Pomponius Atticus and, among the Greeks, Philodemus, Alexander Polyhistor, Antiochus of Ascalon, Asclepiades of Bithynia, Diodorus Siculus, and Posidonius. Above all, it was the Greek genius for classification and systematization that was naturalized in Rome to help bring order out of chaos in many fields. Rawson studies each of these thinkers, documents their writings in descriptive detail, and synthesizes their theories on medicine, architecture and allied sciences, jurisprudence, agronomy, historiography, antiquarianism, astronomy, astrology, philosophy, mathematical arts, geography and ethnography, theology, and musical theory. Curiously, little work was done on political theory except for Cicero's *De Re Publica* and *De Legibus*. Because of the need for technical vocabulary in the then-fluid state of the Latin language and the urgency for Roman orators, lawyers, and historians to impose order on their productions, the areas most diligently cultivated and in highest repute were *grammatica*, *dialectica*, *rhetorica*. Some fields were exclusively in Greek hands: scientific and mathematical studies, philosophy, medicine, geography, and ethnography, as well as the arts of painting and sculpture. Rawson understandably eschews the entire field of belles-lettres, despite the revolutionary debate in Roman intellectual circles on literary style and content.

Rawson also analyzes the causes of the lack of permanent Roman institutions for intellectual pursuits. Private patronage abounded, and the forerunners of the later public patronage under the empire are already visible in the activities of Atticus, Pompey the Great, and Julius Caesar.

Many clichés are demolished by Rawson's landmark study, which will rank as a classic and will be quarried for further specialized studies. One hopes that Rawson will apply her historical microscope also to the Augustan age, when intellectual life was still free and active, before its decline in the first century, which was lamented by Pliny the Elder, one of the great practitioners of the encyclopedism that was to characterize Roman scholarship of the empire.

MEYER REINHOLD
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B. A. MARSHALL. *A Historical Commentary on Asconius*. Columbia: University of Missouri Press. 1985. Pp. xiv, 342. $29.00.

Q. Asconius Pedianus wrote during the rule of the emperors Claudius and Nero, and he is known to have gone blind in the year A.D. 64. His lost writings are a biography of Sallust, a work entitled *Symposion* (in which he imitated Plato and wrote on the notion that physical fitness contributed to one's health and long life), and a work against the detractors of Virgil. Fragments of his historical commentaries on five of Cicero's speeches still exist. Although these commentaries were written for the education of his sons, they remain most valuable to us. The commentaries were recorded between A.D. 54 and 57 and are based on Cicero's preserved, as well as two of his

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unpreserved, orations. The five speeches on which Asconius comments are *In Pisonem, Pro Scauro, Pro Milone, Pro Cornelio,* and *In toga candida.* Contemporary scholars have viewed Asconius's writings to be only of a grammatical nature. This new work by B. A. Marshall, however, concentrates on the historical events covered by Asconius, and textual problems are only incidentally considered. Thus, seen through Marshall's eyes, Asconius provides information not solely for the period of Cicero but for a substantial amount of the Roman Republic as well.

Marshall's principal purpose is to examine some of the controversies for which Asconius is our source of evidence and to represent the different interpretations of these controversies. First, Marshall takes up the question of the range of the commentaries. Why is there only evidence for five of Cicero's orations? The author concludes that Asconius in all probability wrote on more of Cicero's speeches but that these particular commentaries are the only ones that have survived. Second, the purpose for which the commentaries were written is treated, and, as stated above, Marshall determines that they were written for Asconius's sons. Third, Marshall deals with the question of Asconius's use of sources. It appears that, in addition to using the written sources of his day, Asconius relied on his own memory a great deal. The author makes clear that it must be kept in mind that Asconius's writings were a fairly simple set of notes to be used by his sons in their schoolwork. Last, Marshall discusses the reliability of Asconius, who made a number of factual errors in the text. Marshall provides the reader with a list of sixty-one such mistakes, but he points out that Asconius never intended his work to be used by the public. Thus, it did not have to meet the rules of historical criticism.

In the commentaries themselves, Marshall does a scholarly job in presenting the diverse points of view on issues that are raised by Asconius. Each of his entries states the full range of both primary and secondary bibliographical information. The only weakness of the volume is that the majority of the secondary references are from English authorities. Yet, in all fairness to the writer, he does declare this shortcoming in his introduction.

STEPHEN J. SIMON
Appalachian State University

J. P. SULLIVAN. *Literature and Politics in the Age of Nero.* Ithaca: Cornell University Press. 1985. Pp. 218. $22.50.

It is a commonplace that Roman imperial society was strikingly hierarchical and characterized by an acute awareness of social status. The place of Roman poets and other writers in this hierarchy and the nature of their relationship with other Romans have provided the subject for a number of studies in the past few years. In this book J. P. Sullivan focuses on the Neronian age, which, as he reminds us, was second only to the age of Augustus in literary production. Nero's devotion to the arts, in Sullivan's view, was responsible both for this "literary renaissance" and for his downfall at the hands of disapproving aristocrats.

In the first chapter Nero's artistic interests, his circle of literary friends, and the themes of pro-Neronian propaganda, as well as other literary circles and literary works critical of his reign, are described. The second chapter sets out the debate over the aesthetic principles for poetic composition and places Nero in the camp of Callimacheans, who were opposed to Persius. The third chapter on the Stoic opposition considers the work of Lucan and Seneca, concluding that there was no "monolithic Stoic opposition" (p. 142). Sullivan suggests in the next chapter that in the *Satyricon* the courtier Petronius took the opportunity to attack the literary efforts of Seneca and Lucan after they had fallen from imperial favor. These examples show that "literary life in the Neronian period was intimately connected with patronage, political power, and status" (p. 179) and that the connection with political power distinguished it from literary life in the following era.

Much of this will not surprise classical scholars. Sullivan has little to add to the recent debate on the nature of literary patronage in this era; indeed, he takes the problematic notion of patronage for granted. The Stoic opposition has been treated in more detail and with greater subtlety elsewhere.

As for the political ramifications of literature, Sullivan does not penetrate the surface very deeply, offering arguments that do not exhibit a rigorous method. Anecdotes are adduced as evidence without qualification. The identification of members of Nero's literary circle is largely speculative (for example, the future emperor Titus is included on no better grounds than the general statement by Suetonius about his skill in oratory and versification). Sullivan's image of the Neronian court is not likely to satisfy historians. He would like us to treat Nero and his interests more seriously than did the writers of antiquity, and yet it remains difficult to view as anything other than ludicrous an emperor who returned from a tour of Greece with 1,808 prizes won in contests ranging from singing to a ten-horse chariot race (in which Nero was the only competitor). It may be true that Nero's literary friends enjoyed special favors, but little evidence exists to show "the use of literature as a weapon in the fight for real power at court and in the *consilium principis*" (p. 192). Certainly Tacitus, our best source for the reign, does not give the impression that the

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major political advisers around Nero rose or fell because of their literary tastes or as a result of literary feuds.

The reader will find some thoughtful comments in Sullivan's discussions of works from the period, but many of these discussions seem to be unnecessarily long and to lack point. For instance, five pages (pp. 130–35) are devoted to a description of Seneca's tract "On Clemency," and still the reader gets little sense of the awkwardness inherent in the situation for Seneca, a moralist, who took on the task of praising the virtue of clemency in an autocrat who was known to have executed his stepbrother Britannicus only months before (a fact not mentioned in these pages). This awkwardness of political involvement for a moral philosopher such as Seneca is conveyed much better by Tacitus.

It must be said that the reader can probably find better discussions of many of the issues involving the intersection between the arts and politics of Nero's reign in Miriam Griffin's *Nero: The End of a Dynasty* (1985) or in her earlier book, *Seneca: A Philosopher in Politics* (1976).

RICHARD P. SALLER
University of Chicago

AVERIL CAMERON. *Procopius and the Sixth Century.* (Transformation of the Classical Heritage, number 10.) Berkeley and Los Angeles: University of California Press. 1985. Pp. xiii, 297. $35.00.

Averil Cameron's valuable analysis of Procopius and his literary triumvirate—the *Wars, Secret History,* and *Buildings*—is the first comprehensive work on this sixth-century historian since Felix Dahn's *Prokopius von Cäsarea* (1865). It represents a grand synthesis, a rational scheme for understanding Procopius, his works, the sixth century, and late antiquity in general. Throughout, Cameron's grasp of the subject is masterfully intuitive and rates as intellectual dynamite. Organized into three sections of fourteen chapters, the tome includes four maps, a bibliography, and a useful index. Unfortunately, a list of abbreviations for nonspecialists is lacking.

Revisionist if not revolutionary, Cameron addresses the issues of Procopian perception, reliability, context, and structure. Under her careful scrutiny Procopius emerges not as a classical aberration but as both a sixth-century Byzantine guilty of "Roman" and "barbarian" prejudices and a critic of doctrinal controversies and persecutions. With his classical mask now removed, Procopius is revealed as a believer of the irrational at work in history for whom the miraculous and supernatural replaced historical analysis. When pushed to explain causation, he relied on divine intervention. The *Wars,* though classical in appearance, was written with Christian assumptions and thus is much closer to ecclesiastical than to classical history.

Cameron remains highly skeptical of using Procopius as a historical source, for she characterizes him as "right-wing" (p. 128), nonhistorical, unscholarly, and extremely partial, a staunch conservative and traditionalist, an authoritarian upholder of "law and order" (p. 167), and a total misogynist. Cameron concludes that a naked dependence on Procopius for the history of the period would produce a seriously flawed picture devoid of complex political analysis and sensitivity for the human condition, further marred by unoriginal, repetitive, anecdotal, and simplistic views. So complex an author with such a skewed purpose must be carefully read, with an awareness of his perceptions and the subtleties of context. But Cameron has also isolated Procopius's strengths: he was a skillful observer, a "highly artful reporter" (p. xi), and a good storyteller, whose insightful writing over an event-filled twenty-year period is filled with moments of brilliance and realism.

Most controversial of all is Cameron's convincing thesis, which supports the unified nature of Procopius's three works based on a close chronological development and literary "coherence" (p. x). To read Procopius correctly is to consider what all three works say, or omit, about an issue. This triad represents different Procopian viewpoints, created and constrained by the literary conventions and political climate of the time.

In short, Cameron has successfully brought Procopius out of the academic closet to a wider, nonspecialist audience and, at the same time, has offered to specialists a refreshingly new approach to a crucial sixth-century historical source.

W. J. CHERF
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EDITH MARY WIGHTMAN. *Gallia Belgica.* Berkeley and Los Angeles: University of California Press. 1985. Pp. xi, 385. $45.00.

Although remote to the Mediterranean core of the classical world, the region of Belgica still played an important role in the rise and fall of the western Roman Empire. Julius Caesar made it part of the conquests that enhanced his prestige and allowed him to undermine the Roman Republic in the first century B.C.; in the fifth century A.D., invasions and settlements of barbarians eventually contributed to the demise of Roman political influence in Western Europe. Edith Mary Wightman's survey of Gallia Belgica, the province into which Rome organized northern Gaul, starts and ends at precisely those chronological turning points, with an initial chapter

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on pre-Roman Gaul included to set the stage. Learned, comprehensive, concise, but never overbearing or monotonous, this is an outstanding survey of a single province. The emphasis is clearly on society and economy, with little narrative of political events; urbanization, exploitation of the land, even the production of pottery, are more important than emperors. For information Wightman uses the conclusions of archaeologists to full measure, all the more important since so few literary sources are available for northern Gaul during most of this period.

Three themes are particularly consequential. First, Wightman stresses variety within the province: Roman administration imposed "at best an artificial unity" (p. 1). Geography already distinguished the rugged eastern part of the province that was oriented toward Central Europe from the lowlands of the west and north facing the Atlantic. Roman influence reinforced local variations by promoting the east. There more Romanized nomenclature became common, a villa economy flourished longer, production of fine pottery remained steady, the circulation of coinage promoted a market economy, and cities retained their corporate and economic functions. This boom in eastern Belgica was most apparent in the regions near Trier, and Trier in turn was most affected by the presence of Roman administrators, Roman troops, and eventually an imperial court whose demands promoted the production and marketing of agricultural and manufactured surpluses. For modern historians Wightman's careful distinctions provide both a model for research on other regions and a warning against superficial generalizations about other entire provinces.

A second important theme concerns the impact of Romanization. Wightman discusses the role of military service and the (admittedly limited) opportunities of service in the imperial administration for the acculturation of Gauls into the early Roman Empire, as well as the imposition of an urban life style characteristic of the Mediterranean south. But she also emphasizes the survival of indigenous customs and relationships: even the public buildings of cities preserved native architecture. Until the later second century, benign indifference characterized the relationship between Belgica and the empire. In the end, although Wightman insists on the lasting importance of the Roman contribution, her own demonstration of an underlying continuity from pre-Roman through Roman to early medieval society does not adequately support this assertion. Instead, even after Diocletian multiplied the number of administrators by dividing Belgica into two provinces and after more Gauls began to serve in the imperial bureaucracy, "at the local level, provincial capitals apart, little had apparently changed" (p. 204).

The limited impact of Romanization has implications about a third important theme, the rise in importance of Northern Europe at the expense of the Roman, Mediterranean south. Henri Pirenne once argued that even northern Gaul remained "Roman" until the expansion of Islam during the seventh century and the transition to the Carolingians in the eighth century. Although never mentioning the Pirenne thesis, Wightman hints at a drastic revision: not only was Roman Belgica, which eventually became part of the heartland of the Carolingian kingdom, inadequately integrated into the Roman Empire, but pre-Roman Belgica had already revealed "medieval" characteristics such as a landed nobility living in hill forts, warrior bands engaging in endemic petty conflicts, and an exploited peasantry. The imposition of a Roman administration and the development of an urban life style did not effectively subordinate the north to the south. During the third century the appearance of an independent Gallic empire publicized "a tendency towards separation" (p. 198) that had always existed; during the fourth century some Franks were already influential at the western imperial court; during the fifth century, private war bands of Romans and barbarians carved out small kingdoms. "The whole balance of the Empire . . . was shifting further north" (p. 215). Wightman generally implies that medieval society appeared early, rather than that Roman society lingered late, in northern Gaul.

By touching either directly or by implication on so many of the important issues in the development of the western Roman Empire and early medieval Europe, Wightman's magnificent survey marks a fitting conclusion to two decades of important research on northern Gaul. And, although she had begun working on Roman North Africa, her tragic death means that *Gallia Belgica* must also serve, sadly for us, as her final legacy to our understanding of the western Roman Empire.

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MEDIEVAL

RUDOLPH M. BELL. *Holy Anorexia*. Epilogue by WILLIAM N. DAVIS. Chicago: University of Chicago Press. 1985. Pp. xii, 248. $22.50.

Although masochistic behavior had often led to sainthood in the early Christian church, moderation triumped in the West with Saint Benedict in the sixth century. The new moderation, however, made it more difficult for women to become saints since

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they lacked the opportunities that men had in church administration and leadership or through teaching and missionary activities. Rudolph M. Bell's thesis is that the definition of saint underwent a change in the thirteenth century with the development of the mendicant orders. The result was a new emphasis on personal responsibility, and this enabled women to assert themselves in the name of God. Women ranging from Saint Catherine of Sienna in the thirteenth century to Saint Catherine of Racconigi in the early sixteenth century did so through a behavior pattern that Bell labels "holy anorexia."

The 261 saintly women included in the study often sought to control their own destiny by literally starving themselves in their ultimate quest to become brides of Christ. In this quest their bodies became painful reminders of earthly realities, and they fought their physical desires by ever-escalating physical tortures, including denial of the stimuli of hunger and fatigue. As they conquered their physical needs, they achieved a mystical union with God but found themselves unable to sleep or eat except for partaking of the Host, which symbolized their union with God. Most died before they reached the age of forty.

This pattern existed in Italy (Bell concentrates on Italian saints) for nearly four centuries. Half of the 42 Italian women who became saints in the thirteenth century followed this pattern, and a significant number of those in the next three centuries did so as well. With the advent of Martin Luther, however, the church became more suspicious of the female as evidenced both by the witchcraft trials and by the establishment of the all-male Jesuit order, and women found it more difficult to achieve sainthood by masochistic behavior. It was not until the eighteenth century that the ideal of female holiness changed to that of the do-gooder, and a new kind of woman saint emerged.

Bell includes a chapter on the modern definition of anorexia nervosa, indicates in what ways the medieval saints were similar to each other and how they differed, and makes, in my opinion, an effective argument for his definition. The book, based on archival research in Italy, is first-class social history and is well documented both in its historical and scientific portions. There is a postscript by William N. Davis, the director of the Center for the Study of Anorexia and Bulimia at Cornell. Although Bell inevitably uses psychological theory, he lets the data speak for itself and emphasizes the difference between the medieval anorexic saint and the modern anorexic. Notes, bibliography, and index are all helpful.

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JOHANNES LAUDAGE. *Priesterbild und Reformpapsttum im 11. Jahrhundert.* (Beihefte zum Archiv für Kulturgeschichte, number 22.) Cologne: Böhlau. 1984. Pp. viii, 338. DM 94.

Medieval historians have not been conspicuous in demanding a new book on the investiture controversy, one of the most continuously studied, hotly debated, and, probably, irreconcilable problems in all of historical scholarship. Johannes Laudage is brave to enter where so many have at their peril preceded him, and he emerges, I think, having conquered some little part of the field for himself. His contribution is valuable, but his book is not going to transform dramatically the major currents in interpretation. In fact, one of the few dissatisfying aspects of this book is its author's inability to decide whether he embraces or rejects the main contours of the historiographical traditions within which he is working.

Laudage's title accurately indicates that he enters the complex world of eleventh-century religious turmoil through the image of the priest and of the priesthood. He argues that as early as Burchard of Worms and Pope Benedict VIII—in the early eleventh century—a new and ever more coherent picture of the priestly office began to develop. Some features of this picture were traditional, drawn from patristic and Carolingian sources. One element, however, was original, and one of Laudage's real contributions is to have spotted it and to have worked out its implications. Those who were articulating a new priestly ideal drew a tight connection between the quality of priestly life and the validity of the priest's sacramental office, which was itself being put at the center of that priestly ideal. Laudage's key achievement, though, is his demonstration that the new ideal emerged early and widely and that this ideal was not a product of the reform but rather one of its fundamental inspirations. I find his arguments very persuasive, but I suspect that not all will do so.

As to the ideal itself, a priest must live well, give no moral offense, avoid the trappings of lay life, attend honorably and reverently to the sacraments, and achieve a high level of spiritual and intellectual formation. To accomplish these objectives lay interference, increasingly identified with simony in all imaginable instances, had to be avoided. Because of his sacramental office a priest was functionally different from a layman and also in some respects superior. That principle had to be asserted, recognized, and guarded and was all the while rich with potential for confrontation. Still, the early reformers were not confrontationists and confrontation was not inevitable. Little in this ideal was really original, but new was the configuration these ideas assumed in the minds of the reformers who began gathering in Rome after the Synod of Sutri.

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By focusing on the priestly ideal and its prominence in generating, sustaining, and even defining the reform, Laudage is able to add another voice to the chorus of those who see the eleventh-century reform as touching the whole church, as involving more than the struggle between *regnum* and *sacerdotium*, and as having more than monastic inspirations. More could have been done with the German and Italian nobilities and towns. Comments on France are too brief to be useful. England is ignored (as is most English-language scholarship). But one can only do so much, and Laudage has written a subtle, interesting, and valuable book.

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MARJORIE CHIBNALL. *The World of Orderic Vitalis*. New York: Clarendon Press of Oxford University Press. 1984. Pp. viii, 255. $29.95.

Having completed in 1980 a magisterial six-volume edition and translation of the *Ecclesiastical History* of Orderic Vitalis, Marjorie Chibnall now returns to gather up the threads. In four groups of chapters, she tries to put Orderic within the environment that shaped him and his vast work. The first section treats Orderic's life (1075–ca. 1142) from boyhood in England through the long years at the Abbey of St. Evroul in Normandy. Next come three chapters, perhaps the best in the book, that describe the monastic world he knew. A third part turns to the secular world as far away as the Levant. Then, in the final cluster of chapters, Chibnall reflects on Orderic's powers as a historian and on the fate of his work.

She sees in Orderic something of what another great editor, Charles Plummer, saw in the Venerable Bede, Orderic's most admired medieval predecessor: she writes as if Orderic were almost her colleague, struggling earnestly, as does she, to reconstruct events in a true record. Yet she has done more than Plummer to measure the distance between medieval historiography and modern historicism. In fact, she cites with apparent approval Peter Wiseman's explosive book *Clio's Cosmetics* (1978), which is no grist for a mere positivist's mill. In a few essays on historians of the late Roman Republic, Wiseman suggested a way of talking with new clarity about the methods of the more copious Western historians both before and long after the late Republic. They generated at least some of their materials—both words and deeds—by a method more analogous to rhetorical *inventio* than to modern research: by imagination and the rule of verisimilitude, not empirical inquiry and the standard of evidence. Chibnall acknowledges that classical rhetoric was still a force, however attenuated, in twelfth-century historiography, especially in books such as John of Salisbury's *Historia Pontificalis*. She cites its impact on some, though not all, of the lively patches of direct discourse in Orderic. But it is not clear to me that Orderic would have been greatly more hesitant than John to imagine both speeches and actions

if his information was insufficient to his purposes.

Perhaps Chibnall underrates the fact that Orderic's beloved claustral teacher, John of Rheims, came to St. Evroul from the cathedral school at Rheims, where, as she points out, the study of the Latin verbal arts flourished in the eleventh century. Her book leaves one to ask how far the *Ecclesiastical History* was a work of rhetorical art or, to borrow Wiseman's term, "creative historiography." It is a fundamentally important question that can now be addressed out of the abundance of Chibnall's thirty years of fruitful work on Orderic.

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KASPAR ELM, editor. *Norbert von Xanten: Adliger, Ordensstifter, Kirchenfürst*. Cologne: Wienand. 1984. Pp. 357. DM 48.

In commemoration of the 850th anniversary of the death of Saint Norbert, the community of St. Victor in Xanten commissioned Kaspar Elm to edit this volume. The result is a well-thought-out book with relatively little repetition among the eight authors. The stages of Saint Norbert's development are distributed among several essays, and there is some treatment of the later history of the Premonstratensian order as well as of the remembrance of Norbert. Although the notes are placed at the end of each essay, little expense has been spared, and the *Festschrift* is illustrated with dozens of pictures and maps, as well as with reproductions of "St. Norbert of Xanten, 1134–1984," the exhibition organized at Xanten by Géza Jáczai.

A short introduction by Elm leads into five essays that trace Norbert from Rhenish noble to canon, itinerant preacher, founder of the Premonstratensians, and finally archbishop of Magdeburg. In a piece too short to serve as a reference work for the critical problems involved with the Norbertine sources but abreast of recent scholarship and intended as a bibliographical guide to this scholarship, Wilfried M. Grauwen discusses the sources for the life of Norbert and the history of their study. Because Norbert was a man of the spoken, rather than the written, word, he must be studied through the writings of his contemporaries. Indeed, the cumulative effect of critical scholarship has been to leave little written evidence correctly attributed to Norbert himself. Grauwen properly warns us that many standard works continue the misascription of works to Norbert. His useful essay, which among other

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things summarizes his own publications in Dutch, is followed by a transcription and translation of the beginning of the Vita Norberti B.

The next article, by Alfons Alders, is on Norbert as Rhenish noble and canon at St. Victor. This full presentation of Norbert's youth and early years continues the critical assessment of the two vitae of Norbert. His career is clearly situated within the larger ecclesiastical movements of his day, but attention centers on the reconstruction of concrete events, such as his famous conversion. Alders places Norbert's religious development within the most current scholarship. Franz J. Felten, in a substantial essay, next covers the eight years from 1118 to 1126, in which Norbert tasted almost the whole range of early twelfth-century religious experience. In 1118 his life was radically oriented around the ideal of the *vita apostolica*, expressed in poverty and the life of the barefoot traveling pilgrim-preacher. By 1126 he was an archbishop and imperial prince. In between, he had founded Prémontré. Felten's reconstruction of these years, which is frequently critical of earlier scholarship, is convincing. Clearly Norbert held many inchoate and sometimes contradictory spiritual ideals, which were only given a clear institutional expression by Hugh of Fosses some years after Nobert had become archbishop.

Stefan Weinfurter goes over some of the same ground in the next essay, on Norbert as reform canon and founder of the Premonstratensian order, but he demonstrates a knowledge of medieval theology that greatly illuminates Norbert's work. A reproduction, transcription, and translation of the opening of the *Praeceptum* in MS 101 of the Universitätsbibliothek Münster precedes an essay without notes by Berent Schwineköper on Norbert as archbishop of Magdeburg, which closes the group of essays on Norbert's life.

Felten makes a second useful contribution with a map and short article on Norbert's journeys. Renate Stahlheber then traces the iconography of Norbert into the modern period. Ludger Horstkötter also brings the story down to modern times in tracing the Premonstratensian cloisters on the lower Rhine and in Westfalia. Finally, Elm gives us a useful synthetic article on Norbert's significance, personality, and influence. He reminds us of the closeness in historical memory of Norbert and Bernard of Clairvaux, with Norbert consumed with the mission to reduce the discrepancy between the life of the church and the Gospel ideal. Barefoot and in rough clothes, he was the preacher of repentance. With the life of Augustine as his model, he was not so much the founder of a religious order as someone called to reform existing life back to the life of Christ, the apostles, and the *ecclesia primitiva*.

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DAVID HERLIHY. *Medieval Households*. (Studies in Cultural History.) Cambridge: Harvard University Press. 1985. Pp. xii, 227. Cloth $30.00, paper $11.95.

The central thesis of this immensely stimulating book is that the medieval household evolved from the seventh century as a distinct type of family unit commensurable across all social levels, although the households of the wealthy were larger and more structurally complex than those of the poor. The nuclear household became identified with the single-family farm during the early Middle Ages and always retained this characteristic. It had strong emotional and moral ties with the kindred. Although family consciousness was cognatic through the seventh century, it had become almost exclusively agnatic by the eleventh century, with the consortial and dynastic lineages as the dominant forms. Church law enforced monogamy, stronger patrilineal consciousness, and exogamy, for even affinal marriage was prohibited. Expressly contesting Jack Goody's argument that the church tried to hinder marriage in order to facilitate its own property acquisitions, David Herlihy claims that the broad definition of incest by the early medieval church came from its desire to limit resource polygyny, the monopoly of marriageable females by powerful clan heads.

Herlihy's pioneering studies of the Tuscan household naturally figure prominently in this book, and he uses recent studies of English villages for comparison. He finds that rules of land tenure were more important than inheritance law in determining the ability of households to keep integral farms. The wealthiest one-quarter of households raised one-half the children, for they had more children than the poor, and many young persons entered the wealthier households as servants or poor relations, then exited at their maturity, generally by moving downward economically. The medieval household was thus characterized by a considerable lateral extension but minimal vertical extension.

The book explains the fluctuating fortunes of women within the household. In the late Roman period many women had made free marriages. Corresponding to this, the reverse dowry, of husband to wife, outstripped the value of the dowry from the third century onward. But by the central Middle Ages women outnumbered men, and men had economic power. Thus, the financial burden of marriage shifted back to the woman's family. Women married early, often to men considerably older than themselves. The changes of the fourteenth century encouraged earlier marriages and smaller families, but in the fifteenth century, as population again expanded without a corresponding economic growth, age at marriage and house-

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hold size both rose again. An internal "self-regulating mechanism" (p. 144) had developed by about 1300 for the households of the upper classes but not for those of the lower, which continued to produce children too rapidly for their means of support. As standards of living rose in the fifteenth century, however, more persons acquired land and capital, and the "last Malthusian crisis" passed (p. 145).

One cannot call this a work of synthesis, for it contains a vast amount of original scholarship. Herlihy has a rare talent for incorporating lively narrative evidence into the context suggested by quantifiable data and writing about it with grace and verve. Inevitably, specialists will question points of detail. His analysis of the polyptych of Irminon in terms of the conclusions suggested by the Tuscan *catasto* will make some uncomfortable. But this book will provide the analytical framework against which all subsequent regional studies will be tested.

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RICHARD M. SMITH, editor. *Land, Kinship, and Life-Cycle.* (Cambridge Studies in Population, Economy, and Society in Past Time, number 1.) New York: Cambridge University Press. 1984. Pp. xiii, 547. $59.50.

This wide-ranging collection of fourteen essays is disappointing. Its parts are far better than the whole. A rambling, sometimes incoherent introduction leads the reader into the minutiae of the social history of fifteen thousand English villages and fifty thousand feudal manors and into the lives of obscure villagers, but the reader is never quite sure why it is important to know this micro-level information. Furthermore, even though most of the essays display substantial technical virtuosity in extracting this information from recalcitrant sources, I had the impression that rote scholarship has found its highest expression between these covers. I am not sure what purpose is served by such a relentless focus on the minutiae of obscurity without some sense of context.

In the index to this book one finds mention of neither "feudalism" nor "capitalism," but fifteen Norfolk villages figure prominently. In truth, the collection suffers from an evasion of the historian's primary goal, that is, the explication of the past and how it developed into the present. Saying this is neither to underestimate the skill of the contributors nor to downplay the importance of their individual findings—and some are of real importance to specialists—but rather to insist that this collection is without direction and wanders aimlessly. It has no coherence, largely because its willingness to lay down before the altar of academic centrifugality essentially disassociates the contributions from the larger flow of the historical process in which each essay must be situated. On finishing the collection, I realized that it was rather like reading through two journals—the one a special issue on the explication of medieval manorial court rolls and the other a hodgepodge on the rural scene in the early modern era. Each contribution was up to the mark; each contribution had some important things to say; each contributor met the expected standards. Taken altogether, however, the collection said nothing. So, it seems to me that the logic of this collection lies elsewhere, and, indeed, it is easy to find evidence of an editorial thrust to reduce family history, during the transition from feudalism to capitalism, to a demographic lottery. The concept of a "demographic lottery" is certainly useful, but it is, I firmly believe, subsidiary to other themes. The family exists in history and society, and only if one believes that both history and society are simply "systems" can one reify a concept such as "demographic lottery" and place it in the center of the stage. Without a guiding notion of the political economy of English history—remember neither feudalism nor capitalism are found in the index—one is left to the anarchy of concepts such as the "demographic lottery" to explain the orbit of family history. It just will not do.

I do not want to end on a sour note since several of the individual contributions are worthy of mention: Bruce Campbell's study follows peasant survival strategies in a period of extraordinary population pressure before the Black Death; Ian Blanchard's musings on the changing configuration of family organization—stem and nuclear, patriarchalism and individualism—among industrial and agricultural peasants tick like a time bomb in this company; Tim Wales's and W. Newman Brown's essays on poverty and poor relief in the seventeenth century illuminate a little-understood subject; and Dennis Mills's fine discussion of the peasantry in Melbourn, Cambridgeshire, in the nineteenth century should, one hopes, firmly put to rest the silliness surrounding Macfarlane's epitaph for a "non-existent peasantry." Yet these individual specimens of craft—and some of the others that I have not singled out for special mention—did not persuade me that the collection as a whole had a purpose, unless, of course, that purpose was to suggest that the history of the family takes place outside the historical process. Of that, I remain unconvinced.

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GRAHAM TWIGG. *The Black Death: A Biological Reappraisal.* New York: Schocken. 1984. Pp. 254. $23.50.

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We all know a lot less about plague than we think we do, and historians know a lot less about fourteenth-century Europe than they would like to know. Hence, the frequent attempts to explain in modern terms the great fourteenth-century epidemic(s), known in English as the Black Death, are usually exercises in ploughing the sands. Because plague is a vector-borne disease with a complex etiology that grows more complex every year, historians with little or no scientific training who discuss its history usually get one or more essential aspects of the disease wrong, sometimes because of an undue anxiety to make sense out of their highly defective historical sources. They become devotees of the Aristotelian "improbably possible"; epidemiology, however, is the science of the probable. Scientific workers on plague are rarely tempted into this bog of imprecisions.

It is therefore refreshing to have Graham Twigg, a zoologist specializing in the biology of rodents, enter the controversy. He has some useful things to say about the etiology of plague of which historians need to be reminded. The fourteenth-century evidence for the epidemic being one of plague is poor because what is reported is often inconsistent with the known characteristics of plague. Specifically he poses some important questions. First, how could ships, whose voyages lasted four to twelve weeks, carry plague sixteen hundred and twenty-two hundred miles (from Caffa to Genoa) without some crew members dying from plague on the voyage? After the ships touched shore, how could plague then immediately cause a universal and rapidly spreading epidemic in European cities and produce high instantaneous mortality before it had time to establish itself in the local rodent population? Second, how could plague, dependent as it would be on the agency of the black rat and the plague flea (*Xenopsylla cheopis*), spread throughout Europe in a cold climate? These carriers, because of their dependence on man for warmth in temperate and cold climates and the rat's reluctance to move about, would be confined to buildings and, hence, would not be able to disseminate the disease widely and rapidly over the land, especially in conditions of sparse, widely dispersed human population. Third, how could plague in the fourteenth century spread just as rapidly and universally in the cold winter months as in the warm summer ones?

To these questions some historians will no doubt reply that the plague must have had a different etiology in the fourteenth century—one suitable to rapid interhuman transmission and maintenance. Therefore, to reach a reasonable answer, we must ask what evidence shows that plague's etiology was different? Can it be established in acceptable scientific terms by what evolutionary prodigies a disease that was supposedly demic in the fourteenth century has become zootic in the twentieth?

Alas, Twigg's analysis is more effective in its plague biology than in its history. His dependence on secondary works (most of the old stand-bys of previous historians) means that he has missed some good clinical evidence, notably, Guy de Chauliac's contemporary distinction between plague and anthrax (which Twigg rather hastily identifies in the last ten pages as a more probable candidate than plague for the Black Death). His somewhat-parochial approach has led him largely to ignore the Black Death in Europe outside England. The work seems to come apart toward the end. It is as though the author gradually becomes aware of two awkwardnesses with which he will have to deal if he keeps on. First, where in Europe does the Black Death (acknowledged by Twigg to be plague in the Mediterranean lands) become anthrax? And when does the great epidemic disease of Europe (accurately and consistently identified as plague from the late sixteenth century to the twentieth century) cease to be anthrax or other diseases?

Nonetheless, this is a most useful and stimulating work. The sections on rodent and flea biology by themselves provide a salutary update—and a reminder that, for more than a century, disease and its history have been matters for scientific analysis.

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JESSE M. GELLRICH. *The Idea of the Book in the Middle Ages: Language Theory, Mythology, and Fiction.* Ithaca: Cornell University Press. 1985. Pp. 292. $27.50.

Jesse M. Gellrich begins this book by stating that he will reconsider the place of literature among the cultural forms of the Middle Ages in light of "proposals in critical theory which have emerged since structuralism" (p. 17); he concludes by stating that "meditations on this body of literature take us back to the evidence of deconstruction that was under way many years before the 'discovery' of the movement in the twentieth century" (p. 255). Not surprisingly, then, the ideas of Jacques Derrida, Michel Foucault, and Claude Lévi-Strauss figure prominently in the book. Gellrich describes his overall aim as the examination of the nature of historical change insofar as it may be reflected in linguistic change, using ideas of text to explore mythologies of medieval culture.

The book is divided into seven chapters and a brief retrospect, "On Historical Change." The first chapter deals with medieval concepts of "the book," the second with comparisons among architecture, scholasticism, and music, and the third with medieval grammar, paying special attention to the study of

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signs by the speculative grammarians. In a chapter on Dante the author argues that the *Divina Commedia* contains two contrasting ideas of "the book." Three chapters on Chaucer complete the substantive part of the volume, with discussions centering on the *House of Fame*, the prologue to *The Legend of Good Women*, and the general prologue to *The Canterbury Tales*.

Gellrich amasses an impressive number of medieval statements about writing, about signs, about "the book," and about the cultural role of the Bible. Although he notes at one point that his efforts "can only be to assemble suggestive evidence" (p. 20), the whole book does seem to rest on an unstated assumption that, if we can only marshal the requisite number of citations, we will have the inductive data for the conclusion that there was a single archetypal idea of "the book" during the Middle Ages. That assumption is indicated even in the study's title and is confirmed throughout the chapters by the quest for "continuity," even while the author wrestles with the variegated forms used by Dante and Chaucer. Unfortunately, this tension is not faced directly in this volume.

Perhaps the key to this argumentative flaw can be found in chapter 3, titled "The Language of Mythology: On Medieval Grammar and Hermeneutics." Commendably enough, the author strives to separate a potential cultural mythology from the formal educational apparatus of the *trivium* of grammar, dialectic, and rhetoric. He concentrates only on grammar, however, ignoring most of the scholastic discussions of language based on dialectic and ignoring rhetoric completely. Consequently, there seems to be no awareness on his part that all the cited writers could be engaged in *amplificatio* rather than philosophical pronouncement. His radically selective reading of Saint Augustine's *De doctrina christiania*, for example—based, like D. W. Robertson's, only on the first three books without the fourth—enables him to conceive of a monolithic medieval mythology that is awaiting our discovery. Such an entity may be discoverable, of course, but this book simply dismisses the possibility that several hundred men writing over a thousand years may have been writing about somewhat different things.

This is indeed an interesting start toward the goal of assembling "suggestive evidence." At the same time it demonstrates how distant that goal might prove to be.

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CHARLES M. RADDING. *A World Made by Men: Cognition and Society, 400–1200*. Chapel Hill: University of North Carolina Press. 1985. Pp. xi, 286. $28.00.

Charles M. Radding is interested in exploring the changes in man's way of thinking about the world that took place in the late antique period and again in the eleventh and twelfth centuries. He consciously uses the line of reasoning employed by the French psychologist Jean Piaget and thus can say that he has traced "how changes in cognitive processes in the first centuries A.D. severed many classical traditions of thought. Magic, both Christian and pagan, reigned supreme in attitudes toward the natural world, while in social thought conceptions of peer relations began to disappear, replaced by an ideal of hierarchy and authority" (p. 255). Thus, man's cognitive processes shifted from a questioning way of looking at the natural world to a way of thinking that took the world as a manifestation of God's will; no natural explanations were sought.

Radding explains this change as a combination of the effects occasioned by the shift in political power from the personal, local level to the impersonal imperial government, the loss of confidence caused by the problems of the late empire, and the otherworldly Christian way of looking at things. Thus, the mental outlook of western Romans had become little different from that of the primitive Germans who had never risen above such an outlook, and fusion of the two could take place with little difficulty. Early medieval men "looked for direction to the pronouncements of superiors—kings, abbots, or the ancients—whom they tended to regard as divinely inspired. . . . They rarely concerned themselves with intention, whether they were assessing the morality of an action or interpreting the words of authority" (p. 153).

Radding then argues that a change in this kind of mentality occurred in the eleventh century, beginning at the cathedral school of Chartres under Fulbert and at the law court in Pavia. This change was produced by the combination of a long-term improvement in education and the collapse of the political order in many parts of Europe so that there was no longer an authority to which an appeal could be made. Then man began to question the natural world again, regain confidence in his own intellect, and become capable of logical thinking that went beyond mere rote memorization of categories.

Radding has provided us with an impressive reinterpretation of medieval intellectual history based on his conviction that man's social context is determined not by external factors but by his internal way of looking at the world (in other words, ours is "a world made by men"). This approach has a certain logical appeal, but explaining changes in man's way of looking at the world is not easy. The shift in the late antique world seems fairly obvious, but that in the eleventh century is not. The appeal to a breakdown of authority is attractive, but outside the area with which Radding is most familiar

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(France) it seems to lose effect. His treatment of the importance of legal studies at Pavia is a case in point. Disappointment over the removal by the Saxon rulers of the court from Pavia hardly seems to justify the argument that it was a breakdown in authority that was the stimulus for the development of legal reasoning there, precursor to the work of the legists at Bologna.

But this is a provocative book. Well written, thoughtful, it will stimulate medieval intellectual historians to respond in kind.

K. F. DREW
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TINA STIEFEL. *The Intellectual Revolution in Twelfth-Century Europe*. New York: St. Martin's. 1985. Pp. 117. $19.95.

Although the subject of this book is fascinating and of major importance, Tina Stiefel has proved to be completely unable to deal with it. From the introductory section on classical Greek thought to the concluding false question of why the twelfth-century movement "failed," the book is marred by strings of inaccurate clichés, by ignorance of the texts and of the best recent scholarship, and by the kind of naiveté one often encounters in undergraduate theses.

Stiefel seems to think that she is engaged in a revisionist study. Her "thesis" is that "productive ideas concerning nature as a fit subject for objective inquiry were articulated in Western Europe before the appearance of the Aristotelian corpus in translation" (p. 2). She could have found much the same point of view expressed, subject to suitable limitations, in many studies of the past thirty years, including a number of my own, so I am not out of sympathy with what she is trying to do, only deeply disappointed over the way she has botched it.

Stiefel restricts her investigation to northern France, almost to Chartres, ignoring the related developments in southern France, Salerno and Sicily, and the northern Italian cities. Although her chief sources are Adelard of Bath, William of Conches, and Thierry of Chartres (p. 109), she does not seem to know Brian Lawn's corrections to Adelard's biography or Nicolaus Häring's edition of Thierry's *De sex dierum operibus*, and she misreads William of Conches's coinage "elementatum" as "elementum," thus missing the whole point of it. Of less importance, but still betraying a lack of knowledge of the best editions, she continues to misspell Calcidius as Chalcidius, despite J. H. Waszink's masterly establishment of the correct spelling.

Her method is to take isolated quotations out of context and overinterpret them in order to make her points. She does this not only to her twelfth-century authors but also to modern scholars, several of whom, I fear, will be appalled at what she represents them to have said.

One could go on at much greater length, but written words cost money to print, and this study does not merit any further attention. It is a book to be shunned.

RICHARD C. DALES
University of Southern California

DAVID STEPHENSON. *The Governance of Gwynedd*. (Studies in Welsh History, number 5.) Cardiff: University of Wales Press; distributed by Humanities, Atlantic Highlands, N.J. 1984. Pp. xlii, 257. $37.75.

David Stephenson has made an important contribution to the limited but high-quality scholarship on medieval Wales. The theme of his book is precisely denoted in the title. Under Llywelyn ap Iorwerth (ca. 1188–1240), Dafydd ap Llywelyn (1240–46), and Llywelyn ap Gruffydd (1246–82), Gwynedd—the extreme north and northwest, essentially corresponding to the modern counties of Gwyn and Clwyd—emerged as the best governed of all the constituent parts of Welsh Wales. Stephenson is concerned to delineate the institutional and personnel bases of that governance rather than to retell the story of the princes' efforts to establish first preeminence and then hegemony over all of Wales, the process irremediably shattered by the Edwardian conquest.

The evidence is limited and often fractious: only a handful of charters, for example, have survived, and the lawbooks are notoriously difficult to use. The princes' achievement itself was, admittedly, also limited: the very existence of a regularized and differentiated chancery and other administrative departments is dubious, and arrangements are best described as flexible, which is perhaps a polite term for sketchy or inchoate. Given the restricted resources, both human and fiscal, and given the highly segmented and personalized nature of social organization, what the rulers of Gwynedd did accomplish is noteworthy. Stephenson uses such terms as "ministerial elite" (p. 195) and "state" (p. 186); he is justified in doing so largely because he immediately and properly qualifies them. He wisely eschews any forays into the murky waters of "national consciousness," "community of the realm," and like shibboleths.

The reader is left, then, with much valuable biographical detail on Gwynedd officials and notables; a clear and effective use of much Welsh-language original source material as well as modern English- and Welsh-language scholarship; and an important reminder that the greatest of the Welsh rulers had far more in common with Marcher lords

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and lordship than with English kings and kingship. That in itself was no mean or unimportant accomplishment, and Stephenson's book stands as a thoughtful and authoritative analysis of that feat.

MICHAEL ALTSCHUL
Case Western Reserve University

DAVID WASSERSTEIN. *The Rise and Fall of the Party-Kings: Politics and Society in Islamic Spain, 1002–1086*. Princeton: Princeton University Press. 1985. Pp. xiii, 338. $35.00.

In A.D. 1002 the caliphate of Córdoba was the strongest state west of Byzantium, but by 1031 it had totally disintegrated, and the balance of power shifted irrevocably from the Spanish Muslims to the Spanish Christians. Drawing on an unprecedentedly wide range of written sources in Arabic, Hebrew, Latin, and Spanish, David Wasserstein makes the best attempt yet to explain these fundamental changes. He studies in turn the caliphate under the Mansurid dictatorship and its collapse; the *taifa* successor states and the roles therein of Muslims, Jews, and Christians; what he calls "the beginnings of the Christian Reconquest"; and the Muslim reaction to this down to 1086.

His general conclusions are convincing: the caliphate imposed a merely artificial unity on a collection of naturally independent regions, and its disintegration was simply a reversion to normality. (No parallel is drawn with the period 1975–85.) Moreover, al-Mansur's massive recruitment of Berbers into the caliphal army and his exclusion from it of Spanish Muslims created a ruling caste of African warriors whose dominance was bitterly resented by all other Muslims, whether their ancestors included Romans, Visigoths, Arabs, Slavs, or even Berbers of an earlier wave of immigration. Thus, when the succession crisis opened in 1009, the New Berbers tried to maintain the centralized regime of al-Mansur, from which they benefited, and everyone else resisted them until the whole state disintegrated. Thenceforward, ethnic and ideological differences disappeared: the *taifa* kingdoms were so weak that they would ally with anyone—Arab, Slav, Old Berber, or New Berber—in order to survive.

Less convincing is the link between the decline of the caliphate and the alleged mass conversions of Christians to Islam in the tenth century, since there is no definite evidence of these at all; in fact, almost nothing is known of the southern Christians after 929. In contrast, much evidence, even if as yet inadequately exploited, survives about the northern Christians. It is certain that the idea of reconquering all the peninsula was current in the chronicles of Alfonso III's reign, which continued to be copied and updated in the tenth and eleventh centuries, and that Alfonso, his predecessors, and his successors reconquered much of the Duero plains. How, then, can it be claimed that in the eleventh century "for the first time in three centuries the Christians were, and saw themselves as, engaged in a serious campaign for the restoration of the entire peninsula to Christian rule" (p. 250)? Instead of the "beginnings" of the Christian reconquest, would not "revival" or "resumption" be a more appropriate term? Similar doubts might be expressed about Sancho the Great's grand alliance and expedition of 1014 (p. 251), the reunion of his dominions under his son (p. 108), Alfonso VI's daughters-in-law (p. 272) and the crusading ideology attached to the *Poema de Mio Cid* (p. 263).

South of the frontier, however, Wasserstein's touch is absolutely sure. One day his book may be supplemented by archaeological discoveries and perhaps even by the finding of Ibn Hayyan's lost chronicle. Meanwhile, it is a marvelous piece of research and the best study of the period since *La España del Cid* (1929).

DEREK W. LOMAX
University of Birmingham

BERNARD GUENÉE. *States and Rulers in Later Medieval Europe*. Translated by JULIET VALE. New York: Basil Blackwell. 1985. Pp. xi, 322. $45.00.

During the past quarter-century the numerous translations of books on medieval history make one believe that medievalists who write in English are an endangered species. Furthermore, why translate a work for a specialist who has or should have the linguistic ability to read the original? Such works are frequently translated simply because their authors are well-known medievalists. It is, therefore, a pleasure to welcome a justifiable translation of a book valuable for all students of the Middle Ages. Medievalists who have already read Bernard Guenée's *L'Occident aux XIV^e^ et XV^e^ siècles* need only check the revised and augmented bibliography.

Guenée's work is comparative history at its best because it really is comparative and because the author is extraordinarily well informed not only about France but also about the rest of Western Europe. The introduction and part 1 deal with political nomenclature and those collective ideas and attitudes that underlay the evolving states of late medieval Europe, and he also traces the political ideas used by rulers and the expanding bureaucracies. He assesses the importance of race, language, religion, culture, history, and collective identity for state and nation. He discusses treatises written to educate the virtuous and successful ruler as well as the propaganda, processions, pageants, and court

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ceremonies used to buttress and magnify the power of rulers.

In the next two parts Guenée investigates such realities of late medieval states as their financial resources, the techniques used to obtain revenues and to secure loans, local and central administration, the shift from feudal to nonfeudal warfare, and the growth of more sophisticated diplomacy. He then traces the origins of estates, the rise of representative assemblies, and relations between rulers, estates, and assemblies. He concludes with a wide-ranging discussion of the historiographical debates that have raged over such issues as the consequences of the Italian policy of the emperors for German political unity; the reasons for the origin, failure, and success of various representative assemblies; the origin of the Swiss Confederacy; the political role of civic humanism in Italy; and the end of the Middle Ages in England.

The greatest contribution of this book is its questioning of traditional interpretations and conclusions, many of which derive from the Romantic notions and nationalistic sentiments of nineteenth-century historians. Guenée doubts that the fourteenth and fifteenth centuries were the transitional period from the medieval to the modern state, from feudal to absolute monarchy, from an agrarian economy to capitalism. He does not believe that history really works out this neatly. "The ruler," he writes, "used everything that came his way to maintain and increase his power. He played the feudal role and founded chivalric orders; he exhorted his subjects in obedience and roused their patriotism; he encouraged the proliferation of officials and official posts" (p. 207). Guenée is particularly critical of the famous thesis that has the growing bourgeoisie cooperating with the ruler to destroy the aristocracy and to build the modern state. He argues that the aristocracy not only maintained its power but even augmented it in this period.

No matter how learned and objective the scholar, comparative history rarely escapes being flawed. Guenée too often generalizes from the French political experience and the opinions of French historians. When treating the Low Countries, for example, he talks mostly about Brabant and seldom discusses Flanders, which has a richer and more informative political history. Little is said about the institutions of the Burgundian dukes. That all representative assemblies failed on the Continent leads Guenée to ignore the situation in England, where Parliament survived fifteenth-century political turmoil and Tudor strength. Sometimes Guenée is simply wrong. When he writes that Parliament "is hardly mentioned by Fortescue in his writings on English government" (p. 186), has he forgotten about Fortescue's *Governance of England*? Also, it was burgesses, not knights, who represented boroughs, and it was the wardrobe, not the chamber, that was the preferred financial department of the English kings in the late thirteenth and early fourteenth centuries. Nor can the French royal household of the thirteenth century be regarded as more highly developed than the English. And, surely, harsh political, economic, and social realities explain the revolts and tension of the fourteenth and fifteenth centuries far better than Guenée's belief that "this inclination to revolt stemmed primarily from a more or less vague feeling of disquiet and insecurity (p. 194).

Whatever its blemishes, none is serious enough to undermine this remarkably informative and challenging book, which will surmount the criticism and revision it will stimulate to endure as one of those books that must be read. How fortunate that Juliet Vale translated it so ably.

BRYCE LYON
Brown University

STEVEN B. BOWMAN. *The Jews of Byzantium, 1204–1453*. Foreword by ZVI ANKORI. (Judaic Studies.) University: University of Alabama Press. 1985. Pp. xviii, 380. $42.50.

Compared to the historiography on medieval Jewish societies and cultures in Latin Christendom and under Islam, the Jews of Byzantium (Romaniote Jewry) have been a relatively neglected "stepchild" (p. ix). This results both from the relative paucity of sources and from the shift that occurred when Sephardic (Iberian) Jewish immigrants achieved dominance over the indigenous Romaniote Jewish population and their culture in the early sixteenth century.

Steven B. Bowman's new synthesis of this Jewry from the Fourth Crusade until the Ottoman conquest of Constantinople redefines the contours of the field and brings us up to date on a series of political, social, economic, and cultural issues. Like Joshua Starr's *The Jews in the Byzantine Empire, 641–1204* (1939), he complements the historical narrative (part 1) with a diverse anthology of annotated documentary and literary primary sources (part 2), to which he appends specialized excursuses. Byzantinists and other interested readers now have many more Hebrew sources in translation, and this should be especially welcome to those who teach courses in general medieval and Byzantine history as well as in Jewish history.

Although the author is aware that his is "a preliminary attempt" (p. xiii) to make sense of the sources of this period and "the present work has, perforce, been more descriptive of the sources than interpretive" (p. 204), he sometimes offers important revisions as well. In the first of five uneven but

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well-crafted chapters of historical reconstruction, Bowman argues persuasively that the political turning point of late Byzantine Jewry is not the Fourth Crusade in 1204 but the onset of the Palaeologan dynasty (1258–1453). Imperial policy became more supportive of Jews both economically and politically, even as ecclesiastical influence, now increasingly dominated by monk prelates, became more antagonistic and polemical. The larger implications of this new periodization are that the late Byzantine period becomes a Christian anticipation of the tolerant posture that the Ottomans advanced toward their Jewish minority (p. 199).

Chapters 2 and 3 discuss in detail the evidence for Jewish demographic distribution and communal organization in the period. Jewish communal self-government and the Jews' economic participation in such areas as tanning and silk manufacture are treated as well.

Chapter 4 is less successful in penetrating the cultural world of the Romaniote Jewish elite, and its relationship to the social and political context in which it emerged is not considered. Many features of Romaniote Judaism, we are reminded, are indigenous continuations and developments of ancient Palestinian traditions. The treatment is cursory and more superficial compared to the nuanced analyses in the chapters on political and social history. Similarly, the anthology offers snippets from lengthy literary sources more as documentary evidence than as cultural statements in their own right. To be sure, some of these, such as the "Chronicle of Ahimaaz" (1154), are available in English. Bowman makes no claim to be writing an intellectual history, but this chapter is weaker than the rest. Nevertheless, it will stimulate much-needed further research, for he lists and briefly discusses the important literary sources. The fifth chapter on the Ottoman conquest and the afterword neatly highlight the transition at the end of the Palaeologan era and draw out important historical trends.

Although this book is a rich mine of bibliographical documentation, an occasional lapse is inevitable. Bowman does not note that Joseph Hacker has shown (in Benjamin Braude and Bernard Lewis, eds., *Christians and Jews in the Ottoman Empire* [1982], which is listed in Bowman's bibliography) that there was no chief rabbi of Ottoman Jewry (*pace* Bowman, pp. 185, 199); this argues for continuity of the Byzantine situation of a chief rabbi of Constantinople. Similarly, the brief introductory remarks about early Christian Roman law codes and the Jews (pp. 27–28, 99) would have benefited from a consideration of Jeremy Cohen's revisionist study (*Byzantine Studies* 3 [1976]: 1–29).

The book is well edited and beautifully set, with notes refreshingly at the bottom of the page. An endpaper map of Byzantium itself might have helped the geographically less initiated reader.

This is a book to be read and studied. Thanks to Bowman and the recent scholarship he has synthesized, it is clear that Byzantine Jewry is no longer a stepchild; historians have begun drawing up the adoption papers.

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MODERN EUROPE

RICHARD VAN DÜLMEN. *Theater des Schreckens: Gerichtspraxis und Strafrituale in der frühen Neuzeit.* Munich: C. H. Beck. 1985. Pp. 240. DM 38.

The "theater of terror" into which Richard van Dülmen leads us on a proscenium and backstage visit is the public execution of criminals staged from the sixteenth to the end of the eighteenth century as an awesome presentation of political might. Michel Foucault, whose *Surveiller et punir* (1975) must have given the impetus for this latest book on a much-discussed subject, called this grisly spectacle "a ritual of armed law," a demonstration of the ultimate resources of "the mechanisms of power that frame the everyday lives of individuals." Without achieving Foucault's stimulating formulations (but avoiding also the French master's verbal play, oracular utterance, and heavy interpretation that tend to smother the evidence), van Dülmen provides a finely detailed description of punitive practice in the cities and territories of the Holy Roman Empire from the introduction of inquisitorial trial procedures in the early 1500s to the legal reforms of the late eighteenth century. Liberally sprinkled with passages from the sources (many of them heartrending), van Dülmen's vivid account of a most unpleasant subject moves at a steady pace between squeamishness and sensationalism to his main theme: the *mise-en-scène* of executions as a form of ritual killing in which traditional social objectives of purification, satisfaction, and retribution were combined with explicitly political aims of intimidation and exemplary punishment to produce a grim display of controlled, methodical cruelty.

Court sessions under the inquisitorial method were secret, the most important product of the proceedings being the trial protocol culminating in the defendant's confession, often obtained under torture but repeated later out of sight of the instruments of pain. Judgments were given entirely on the basis of this record. *Urteiler*—those who rendered the verdict—never saw the accused or the witnesses. They were academic jurists sitting on a ruler's high

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bench or serving as legal advisers to urban magistrates, or they were law professors to whom the documents were sent for expert opinions. All this took place behind closed doors. The first open scene in the "theater of terror" was the public proclamation of the verdict, an impressively mounted ceremony, especially in the cases of murder or other heinous crimes, the details of which were—like everything that was to happen later in the spectacle—fixed by the provisions of the *Constitutio criminalis Carolina*, the criminal code issued by Charles V in 1532. After being solemnly consigned by trial judge to executioner—a forbidding figure whose work made him "unclean" (he could marry only within his own and a few other contaminating occupations), thus keeping the stain of blood from the authorities in whose employ he stood—the condemned was taken in a macabre procession to the scaffold and often subjected to additional tortures on the way.

Van Dülmen spares his readers no item in the repertoire of ways in which death could be inflicted with maximum social impact. Methods varied widely according to the crime, the position of the delinquent in society and that of friends who could intercede, his or her conduct during the trial, the degree of contrition shown on confession and displayed to the crowd on first sight of the scaffold. The impression made on this multitude was the aim and purpose of the entire spectacle. As Foucault says, "The main character was the people." By no means passive, crowds intervened in the proceedings, abusing victims when their crimes or behavior had displeased them or pelting the hangman with mud or stones out of sympathy for the condemned. Authorities were most afraid of a bungled execution. Crowds were known to free a prisoner if a rope broke or the axe slipped. In such a case, the entire show had missed its mark. But this did not happen often, because executioners were skilled craftsmen and governments took stringent precautions to prevent mishaps. Too much was at stake. An exemplary killing gone off its script revealed the fallibility of authority, thus frustrating the point of the whole production.

Some useful tables in van Dülmen's book indicate that the number of noncapital punishments in relation to death sentences increased sharply from the end of the Thirty Years' War. Everywhere in the empire the number of men and women put to death seems to have declined relative to the urban population in the late seventeenth and the eighteenth centuries. It is tempting to see progress at work in these statistics. Indeed, one finds it hard to read about mutilations and bones broken under a wheel, about flogging posts and eviscerated remains swinging from the rope, without congratulating oneself on the enlightened penal practices of our own time. But this may be a complaisant reaction. We now have executions in the United States. The public is not invited to them, but the media provide full descriptions. The gruesome details of how it is done and what happens are brought home to an apparently enthralled audience. Opponents and supporters of capital punishment parade with their paraphernalia of candles, signs, and chants. The frantic backing and forthing of last-minute appeals, stays, reprieves makes for chilling suspense preceding the finality of the terminal act. Is this not—although by other means—an "act of ritual killing"? *Plus ça change . . .*

GERALD STRAUSS
Indiana University

EDWARD PETERS. *Torture.* (New Perspectives on the Past.) New York: Basil Blackwell. 1985. Pp. viii, 202. Cloth $24.95, paper $9.95.

Edward Peters's latest book informs us not only that legalized torture is still practiced but also that it is a uniquely Western institution. Neither the primitive savage nor the inscrutable Oriental first thought it up but, rather, the very Greeks who gave us democracy and the Romans who bequeathed us our civilization.

Peters has chosen to trace only the history of "public torture," that is, the legalized infliction of physical or psychological pain on jailed prisoners in order to extract from them evidence or confessions of a supposed crime. He carefully excludes from his study all other "sentimental" definitions. He is not at all concerned here with the related matter of capital punishment. As medieval law evolved in Western Europe, jurists tended less to look for the hand of God in seeking convictions (that is, in the practice of "trial by ordeal," which Eastern societies continued to favor) and sought more to convince the culprit to make a public confession. The most efficient way to do this was by application of torture. The European imperialist powers also transported this peculiar legal notion to their new colonies, inflicting "cruel and unusual punishments" on the natives and even forcing them to adapt the institution to their own heretofore-innocent judicial systems.

It remains something of a sociological and anthropological mystery as to why, suddenly in the eighteenth century everywhere in Western civilization, Enlightenment reaction against torturing criminal suspects, the novel idea that legal torture is ethically repugnant, so rapidly captured the imagination of politicians willing and able to change the laws. By the late nineteenth century it almost became true that, as Victor Hugo confidently proclaimed, "torture has ceased to exist."

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But how premature! Peters appropriately devotes the second half of his book to this terrible irony. Since World War II, torture has indeed returned to us with a vengeance. In this very year, 1986, legal torture is again practiced in at least one-third of all countries registered in the United Nations. Why? Peters offers a cogent analysis, showing how naive in the first place was the eighteenth-century ideal of a benign bourgeois state promising liberty and justice for all. Today that once-blissful dream has turned into a nightmare. Haunted by the fear of political revolution and terrorism, we have revived the ancient notion of *crimen exceptum*, the crime that so endangers the "state" that, to prevent it, we suspend individual rights and ignore human dignity. Western civilization realized just how profoundly it was afflicted by this paranoia when in 1957 French military conduct in rebellious Algeria was first revealed. So now we are beset by a new plague, the psychological "disease of the twentieth century," as Sartre warned. The French term it "Massuisme," after their infamous commandant in Algeria. Peters does not bring it up, but the following imaginary scenario not only could happen but probably soon will, and the answer to its question is self-evident: Some terrorists have finally procured an atomic weapon. They have hidden it in a large population center, which they threaten to blow up unless their demands are met. Just in time, the police capture one of the terrorists who knows the location of the bomb and how to defuse it. What should the police then do to make him (or her) talk?

Torture is a superbly researched book. The author writes with style and passionate conviction. He has presented us with an extraordinarily provocative argument—and one that is very disturbing.

SAMUEL Y. EDGERTON, JR.
Williams College

JOSEPH KLAITS. *Servants of Satan: The Age of Witch Hunts*. Bloomington: Indiana University Press. 1985. Pp. 212. $24.95.

One of the more depressing consequences of the specialization in historical research over the past several decades is the increasing importance of works of responsible scholarly synthesis, not only for a broader lay public but also for specialists themselves. Joseph Klaits describes his book as "an extended essay, reflecting on and synthesizing the extensive recent literature on the witch craze of the sixteenth and seventeeth centuries" (preface). He does this accurately and intelligibly, but he also does considerably more. Successfully setting research on witchcraft in the broader historical context of early modern Europe, Klaits has his own ideas, and he states them clearly and often persuasively. One of the problems of works of synthesis is that the synthesizer often has to fill in considerable lacunae between the areas of specialized research he is describing; this Klaits does well. He is faithful to the research of others and effective in locating it in a larger historical context.

Klaits's first chapter surveys the history of witchcraft theories and scholarship. His second chapter deals with the period 1100–1550; in it he usefully cites the work of Jeffrey Burton Russell, Norman Cohn, and others but also treats climates of religious sentiment and the work on marginal groups by scholars such as John Boswell. Here and throughout the book, one of Klaits's real virtues is his citation of scholarship in related fields on such important topics as sexuality, infanticide, gender differentiation, midwifery, mysticism, and the history of art. Chapter 3, "Sexual Politics and Religious Reform in the Witch Craze," is an excellent account of sixteenth-century persecutions in the contexts of religious and moral reform and what Klaits and others perceive as a heightened mysogyny. Attracted by recent theories of Carlo Ginzburg and others concerning "cultural hegemony," Klaits is nonetheless properly skeptical of their categorical applicability, as he is generally about narrowly based explanations that profess to be universal explanatory models. Chapter 4 treats the social typology of the accused, focusing on the beggar and the midwife, and chapter 5 treats the classic accusers. The sixth chapter, "In the Torture Chamber: Legal Reform and Psychological Breakdown," is probably the weakest, since legal history is obviously not Klaits's long suit (he is certainly not alone among early modern historians in this). Chapter 7 deals carefully with the end of the witch hunts. The notes are adequate, and the bibliography is generally excellent.

Although not every worker in the vineyard will fully appreciate Klaits's portrait of the vineyard and its workers, all scholars interested in this and related subjects should be grateful to Klaits, because he has produced both an ideal book for keeping specialists in intellectual contact with one another and a handy and informative introduction to the subject for novices. Although some important works are omitted from the bibliography (Christina Larner's posthumous *Witchcraft and Religion* [1984] probably appeared too late for Klaits to cite it, but the omission of H. A. Kelly's *The Devil, Demonology, and Witchcraft* [1968] leads Klaits to an unhappy reliance on the muddled notions of William Peter Blatty on exorcism). Klaits's reading has been admirably wide. The integration of pictorial materials into witchcraft studies is long overdue, and the author's ventures in this direction are to be praised, although many more artists than Hans Baldung Grien need to be studied for the importance of this material to be adequately assessed.

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In all, Klaits has done a good job. Avoiding the scandalous and sensational, he has maintained throughout, with sensitivity and economy, an awareness of the uniqueness of the theories and persecutions that have fascinated scholars now for two decades and are unlikely to lose their appeal in the foreseeable future.

EDWARD PETERS
University of Pennsylvania

HELMUT COING. *Europäisches Privatrecht.* Volume 1, *Älteres Gemeines Recht (1500 bis 1800).* Munich: C. H. Beck. 1985. Pp. xv, 665. DM 138.

Helmut Coing's long career as a legal historian is already being crowned by his edition *Handbuch der Quellen und Literatur der neueren europäischen Privatrechtsgeschichte* (three volumes consisting of ten parts). He wrote the book under review here to serve a more modest purpose, which is to provide a one-volume review of *Ius commune*, the subsidiary system of private law in most European countries to 1800. This "common law" served as the matrix for the development of legal systems outside of England (and even in England it continued to be influential in prerogative jurisdiction and other areas outside English common law itself). It was a jurists' law, which developed largely in an academic context and operated to fill the gaps left by custom and legislation.

Coing sets the stage with a brief sketch of the history of legal thought between 1500 and the *Code civil*, examining the successive jurisprudential vogues of humanism, *usus modernus*, natural law doctrine, and Enlightenment. The book concentrates on the doctrine of the *usus modernus* of about 1600. After the first hundred pages or so, most historians will find the book rough going, since the material is arranged topically rather than chronologically. Many will want to have it available simply as a reference book to premodern legal doctrine, since the brief articles are keyed to printed treatises of the sixteenth and seventeenth centuries as well as to articles in the *Handwörterbuch der deutschen Rechtsgeschichte* and the *Recueils de la Société Jean Bodin*. This would be too bad, since Coing has a great deal to offer in the body of this book that bears study in its own right. He rightly points up the often-ramshackle nature of the legal doctrines originally propounded by the *Corpus iuris civilis*, often rendered totally incoherent by the attempts in the *Novels* to amend the traditional law. Much of Roman law never was "received," since it was so profoundly at variance with the institutions and values of Western Europe that no amount of good will could make it work. Coing does an admirably concise job of showing the extent to which the various nations of Europe put the various Romano-canonical doctrines into practice.

Probably the most fascinating part of Coing's analysis, precisely because it is an idea in the process of development, is his underlining of the central role of Spanish legal thought in the sixteenth century. Spanish jurists created the type of family trust (*fideicommissum, Majorat*), which swept Europe in the sixteenth century, and Spanish neo-Scholastic thought contributed to the evolution of the *usus modernus*, which has been almost entirely ignored by modern scholarship. The *usus modernus* itself constituted a conscious retreat from the program propounded by the legal humanists in the early part of the century. Coing is frank about the fact that a great deal needs to be done before we can understand precisely what lay behind the *usus modernus*, and it is obvious that a considerable part of the medieval tradition of Bologna was still very much alive centuries after the French humanists had declared it dead. It is a tribute to a great scholar that he can still spur others on to new discoveries while simply making a survey of the field.

Coing intends to continue this study with a volume on law in the nineteenth century. The cover and dust jacket of this book shift the date (1500–1800) from the volume title to the main title, which could lead to confusion about the contents of the volume.

STEVEN ROWAN
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FERDINAND SEIBT. *Revolution in Europa: Ursprung und Wege innerer Gewalt; Strukturen, Elemente, Exempel.* Munich: Süddeutscher. 1984. Pp. 475. DM 39.80.

During the last thirty years, German historians have not been in the forefront of the discussion of revolution in European history. Ferdinand Seibt's book, which attempts to see revolution as a historical phenomenon over a period of seven hundred years, is therefore doubly ambitious, even though the author analyzes in detail only revolutions from the middle of the fourteenth to the early seventeenth century. Like most writers on this subject, he starts with definitions. For Seibt, revolutions must be relatively short processes, which distinguishes them from evolutions and reforms. They are intentional attempts to change the dominant value systems of the society in which they occur. Marx, Seibt argues, saw this clearly but oversimplified the event by proposing an uncritical universalism that took insufficient account of space and time by projecting into the past the nineteenth-century idea of class as the invariable agent of revolution. The theories of modern social scientists are equally cavalier in their

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disregard of historical differences. Seibt's eventual definition is that "a revolution is the revolt of a subsystem within a functional unit, with the purpose of effecting a structural change and with universalist claims" (p. 37).

What Seibt means is that European civilization was pluralist, with different centers of loyalty and conflicting interests and ideals. By the fourteenth century these centers had developed to the point of "latent revolutionary antagonism" (p. 124). Revolutions were an inherent part of European social and cultural development, and, hence, those who made them and those against whom they were made changed with place and time. Nevertheless, revolutions had certain similarities, which were dependent on the characteristics of European society and history. Thus, revolutions were always directed against the ultimate authority, the monarchy and the church, and against the principal owners of land. They were carried by "middle" groups that could certainly include sections of the nobility. The backbone of revolutionary support always came from towns, which were centers of social and intellectual communication and organization, and there was always a socially downward appeal, as well as a "national" one in which language was likely to play an important role. Seibt makes some especially interesting points about the role of large capital cities in revolutions. Since revolutions are intentional attempts to set up new and universal value systems, their philosophies will always be realist and not nominalist.

In the second part of his book Seibt analyzes seven actual, though mostly unsuccessful, revolutions: Cola di Rienzo in Rome, the Cabochiens in Paris, the English Lollards of 1414, the Hussites of Bohemia, the Protestant Reformation in Germany, the revolt of the Netherlands, and the Bohemian revolt of 1618. One may think the choice of date for the Lollards odd and regret that Seibt preferred the relatively unimportant incident of the Cabochiens to the Catholic Holy League of Paris in the 1580s and 1590s. One may also disagree with some of the author's judgments, for instance, his apparent overvaluation of Calvinist as against Catholic general literature. But he has successfully married a history of mentalities with social and political history within the context of a dynamic view of European civilization and society. His book is full of valuable, even profound, insights, and it is very well written. An English translation would be most welcome.

H. G. KOENIGSBERGER,
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DAVID WETZEL. *The Crimean War: A Diplomatic History.* (East European Monographs, number 193.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. viii, 255. $30.00.

It is only fair to mention that I have recently published a book on this same subject, and with a similar purpose: to provide a brief survey of Crimean War diplomacy that deals with all the major countries and political figures involved. I therefore fully agree with David Wetzel's views about the importance of the war in nineteenth-century history. I also agree in general with his approach and with his conclusions about the results of the war. Unfortunately, I have serious reservations about many of his interpretations and his presentation of critical issues and events.

Wetzel has consciously adopted the assertive and provocative style of A. J. P. Taylor, whose works he quotes frequently. But, whereas Taylor, with all his contradictions, inconsistencies, and delight in tilting at the windmills of conventional wisdom, is constantly offering us fresh insights and challenging our assumptions, Wetzel is more often offering us conventional wisdom (and Taylor's) in the guise of fresh insights, and I found his book more confusing than challenging.

Wetzel informs us that "foreign policy is, after all, the product of deep social and economic forces; it is not crudely manufactured in chancelleries" (p. 84). Yet the greater part of his book deals with how statesmen sought to channel and exploit those deep social and economic forces, and much of it shows to what extent policy is in fact manufactured, crudely or otherwise, in chancelleries.

Wetzel believes that Britain and France fought on the right side in the war, that is, on behalf of the Ottoman empire against Russia, and he appears to accept the Anglo-French wartime view that Russia was a semibarbarous state that had to be expelled from the ranks of the great powers. "The Crimean war," they believed, "was more than a struggle between belligerent states. It was a conflict between two ways of life," like the cold war a century later (p. 159). Wetzel never pauses to consider whether the war needed to be fought at all or whether civilization might not have been better served if Western statesmen had taken advantage of the many opportunities they were offered to avoid it.

Wetzel seems convinced that Tsar Nicholas I was intent on embarking on an adventurous policy in the Near East in 1853 and that Russia had abandoned its defense of the status quo "and washed her hands of European affairs" (p. 46). He is less critical of Napoleon III, whose government he consistently calls "liberal," and he says that after Napoleon's coronation on December 2, 1852 (not December 4), he "no longer had the need for an adventurous foreign policy" (p. 45). He does not make clear why

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Napoleon actually pursued such a policy. Nor does he deal with the extravagant war aims of Palmerston and Stratford, although these were surely prime motivations in their conduct of affairs. He virtually ignores the duplicity of the British government in dealing with Austria's attempts to secure a negotiated peace and speaks only of the government's refusal to swallow a solution "devised behind their backs by the Austrians" (p. 91), a serious misrepresentation of the situation. On Russell's dishonest repudiation of his own agreement with the Austrians for a negotiated peace, he writes, "Russell was always sensitive to the conscience of the nation and took a hard line" (p. 115). Wetzel does not take into account how far Russia backed down in the course of the Menshikov mission; his treatment of the Vienna Note and the Turkish ultimatum is confused and confusing; he fails to explain the significance of the "violent interpretation" of the Vienna Note and how it was used by Britain and France to evade previous commitments. He does not even attempt to explain the crucial Sinope incident but says only that on November 13 a Turkish flotilla had been sent to Sinope "supposedly on a peaceful mission" (p. 94).

Wetzel's critical bibliography is wide-ranging and useful, although I do not agree with many of his evaluations. Inexplicably, he does not list the admirably edited and important three-volume publication of Austrian documents dealing with the Crimean War.

NORMAN RICH
Brown University

L. M. SHNEERSON. *Na pereput'e evropeiskoi politiki: Avstro-russko-germanskie otnosheniia, 1871–1875 gg.* [At the Crossroads of European Politics: Austrian, Russian, and German Diplomatic Relations, 1871–75]. Minsk: Universitetskoe. 1984. Pp. 205. 2 r. 30 k.

This is the first detailed Soviet treatment of relations among Austria, Germany, and Russia culminating in the Dreikaiserbund (League of Three Emperors). L. M. Shneerson, the author of two previous books on Russia and the Franco-Prussian War, surveys Soviet and Western historiography briefly and then in chapter 1 describes the European scene after the Franco-Prussian conflict. Disputing assertions by many Western scholars that Bismarck and Germany after 1871 were cautious and pacific, he depicts Germany's course as militaristic and bellicose: "Bismarck's policy was infused with a spirit of aggressiveness and contained elements provocative of conflict" (p. 13). Shneerson emphasizes Russia's problems, aims, and diplomacy in the early 1870s and argues that previously these were neglected or distorted. Stressing heavy German investment in Russian industry in the 1870s, he affirms Germany's growing economic stranglehold over Austria-Hungary.

Foreign Minister A. M. Gorchakov of Russia is depicted as an able diplomat who was assisted well by his entourage and by envoys such as P. P. Ubri in Berlin and Nikolai Novikov in Vienna. Whereas Gorchakov and his colleagues were Francophiles, Alexander II strongly supported alignment with Germany. Gorchakov remained a firm defender of imperial prerogative: Russia must remain monarchical and autocratic "not only for its own salvation but also to serve as a firm national point of support for the Slav peoples suffering under foreign rule" (p. 75). In such competent if somewhat indecisive hands, Shneerson claims, Russian diplomacy aimed to prevent any further weakening of France while cooperating with the German powers.

In somewhat wearisome detail chapter 2 describes negotiations in 1871–72 among the three eastern monarchies. Russia remained wary, affirms Shneerson, realizing that behind friendly Austro-German assurances lurked various nefarious plans to weaken or destroy Russia. In chapter 3 the author presents the conclusion of the Dreikaiserbund in 1873, in which Germany took the initiative and leading role. It was not a true alliance, asserts the author, since it merely required mutual consultation. Conspicuously lacking, he feels, was a sufficient material base for a close tripartite alliance. In chapter 4, "First Tests," Shneerson carries his story to mid-1875. The Austro-Russian relationship remained the Dreikaiserbund's weakest link since both powers lacked common interests and differed on basic issues. Disputing with a Soviet colleague, N. I. Khitrova, that Russia gained nothing from the alignment, Shneerson asserts that it fostered British caution during the Eastern Crisis and Austro-Russian accords at Reichstadt (1876) and Budapest (1877), which guaranteed Austrian neutrality in the Russo-Turkish War. "Despite shaky foundations," he concludes, "the Dreikaiserbund corresponded to a certain degree and for a certain time with its participants' real interests" (p. 177).

This modest book, without maps, illustrations, bibliography, or index, contributes in a real if limited way to our knowledge about the Dreikaiserbund and European diplomacy of the early 1870s, chiefly by describing the Russian side more fully. Meticulously researched, it is based on abundant primary and secondary materials in several languages. Most valuable for Western scholars without access to Soviet archives are generous citations from the Russian Foreign Policy Archive. Skillful use has been made of Russian and some European newspapers. This relatively objective and copiously footnoted volume will prove highly useful to scholars

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unless they get lost in the dense forests of diplomatic history.

DAVID MACKENZIE
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WOLFGANG J. MOMMSEN and HANS-GERHARD HUSUNG, editors. *The Development of Trade Unionism in Great Britain and Germany, 1880–1914*. Boston: Allen and Unwin, for the German Historical Institute, London. 1985. Pp. viii, 400. $27.95.

This volume, based on a carefully organized conference prepared by the German Historical Institute in London, pursues a valid and timely premise: that despite significant differences in context and, even more, in historiographical tradition, German and British trade unionisms are far less different than conventionally imagined and therefore can only benefit from renewed comparative study. Duly noted are Germany's tendency to link unions with political activity and Britain's to see trade unions as a central and autonomous current in its labor movement. But the volume founds its quest for a more subtle comparative effort on the correspondence of surges of mass unionism after 1880, similar results of strike waves in giving birth to new industrial unions, and other parallels. A number of initial essays are directly comparative, or at least deal with British developments in a more general European context. Thus, Eric Hobsbawm and Sidney Pollard review factors in the "new unionism," and James Cronin and Friedhelm Boll discuss strike waves and their trade union impact cross-nationally. Subsequent individual essays, most by current leaders in labor history in the two countries, deal either with Britain or with Germany. But, even here, careful organization produces a comparative thrust, as, for example, in the finding that dock strikes before 1900 in London and Hamburg, though superficially quite different, conduced to similar basic results.

The book moves from initial general considerations to the inquiries on strike patterns, assessment of economic and political context, and the organizational features of the union movements. This is followed by a somewhat more diffuse section on diverse currents such as syndicalism, Christian unionism in Germany, and "Free Labour" in Britain. The final two sections return to more central topics, the relationships among unions, employers, and the state—with particularly interesting comments on the dilemmas employers and politicians faced in choosing between efforts to repress and efforts to integrate—and, ultimately, the place of unions in labor politics in both countries. Again, the essays in these last sections deal with one country or the other, usually through some intelligent parallel organization but occasionally with some unfortunate gaps, so that a major phenomenon in one case is ignored in the other.

Most of the essays are brief, and some have the quality of a survey. All are sustained by excellent citations, and most are well written. In addition to the initial comparative premise, the volume contributes particularly in discussions (in several sections) of trade union organization—where considerable structural differences did divide the two countries—and in dealing with the interactions of the unions with the other participants in labor disputes. Coverage is certainly admirable, although one might lament the absence of any sustained comparison of social (as opposed to political and economic) context; we learn little about workingmen's lives and how these influenced trade union patterns. For most readers the coverage of German unionism will provide more new data than that on Britain, and this achievement alone is welcome; nevertheless, the essays on Britain not only are refreshing reviews but also provide new insights and intelligently use and critique theories on strike patterns and economic waves. Only a few essays rely on discussions available in more extended works by the same authors. One might wish, finally, for an attempt at summary analysis, which would suggest not only the main lines of similarity and differentiation that the essays collectively reveal but also additional topics, within this time period, that might still be usefully explored (for example, unorganized workers, and attention to German employers commensurate with that given, in several good essays here, to British employers or the German state).

Overall, the volume stands as an unusually stimulating comparative venture that combines attention to the staples of labor history—not only trade union politics and ideology but also benefit programs—with newer sectors such as strike patterns and administrative structures. And, although comparative zeal does not extend to a final statement, most of the essays show awareness of the larger purpose while an introductory comment provides a valid framework. In sum, this book is a stimulating study that amply rewards its initial assumptions and will be useful to labor historians of either country in providing entry to major themes and abundant recent work.

PETER N. STEARNS
Carnegie Mellon University

ROBERT R. LOCKE. *The End of Practical Man: Entrepreneurship and Higher Education in Germany, France, and Great Britain, 1880–1940*. (Industrial Development and the Social Fabric, number 7.) Greenwich, Conn.: JAI. 1984. Pp. xii, 363. $22.50.

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In the provocative introduction to this important study, Robert R. Locke takes on the "revisionists" in the debate over relative national economic performance—both those who deny that Great Britain and France fell behind Germany economically in the late nineteenth and early twentieth centuries and those who accept the reality of this lag but deny that entrepreneurial failure was responsible. In contrast, Locke argues for the traditional view: Great Britain and France did fall behind Germany, perhaps not in per-capita economic growth but in economic "development" (that is, industrialization). Moreover, this relative decline stemmed from poor entrepreneurship that, Locke further argues, resulted from deficiencies in engineering and business education. With his interpretive framework thus defined, Locke then proceeds in the body of his work to describe in rich detail how German business and technical education came to surpass that of France and Great Britain by the early twentieth century.

Whether or not differences in higher education were the main determinant of differences in the economic performances of Germany, France, and Great Britain, the rise of business schools and the academic study of business management between 1880 and 1940 is an important, heretofore little explored subject, and Locke provides the best treatment to date of the German side of the story. In telling that story, he first describes the emergence of a new academic discipline, "business economics," from the pioneering work in cost accounting of Eugen Schmalenbach and others. He then shows how succeeding generations of business economists elaborated a body of business theory, trained teachers for secondary schools, worked to disseminate their ideas on efficient management through journals and professional organizations, and eventually ended up as part of the economic planning apparatus of the Third Reich.

Less complete is Locke's treatment of the French side of the story. He addresses the work and impact of Henri Fayol only in passing and too hastily endorses the negative assessment of French management thought and practice enunciated by Pierre Lemy in the late 1920s. After all, if the origins of Germany's postwar economic "miracle" can be located in innovations in German management in the interwar years, as Locke suggests (pp. 282–83), is it not likely that France's no less impressive postwar recovery also had its roots in the interwar years? Locke at least needs to consider the more nuanced view of twentieth-century French business thought and practice found in recent studies by Maurice Lévy-Leboyer, Patrick Fridenson, Alain Baudant, Henri Morsel, Aimée Moutet, and Richard Kuisel, none of which is listed in Locke's otherwise impressive bibliography.

Another criticism is that in his final chapter, "German Business Economics: The Institutionalization of Management," Locke begins by assuming that, in the twentieth century, good management is synonymous with the application of "business science" and the employment of college-educated managers. He then asserts that, by the 1920s, German business management can be characterized as efficient and that this efficiency can be attributed to the business schools simply because some evidence shows that "business science" was being applied by then (for example, by professors serving as industrial consultants). Such a line of argument is rather circular and involves a faith in the efficacy of academic management theory and training that some will find naive, especially at a time when many are blaming America's business woes on the presence of too many "overeducated" M.B.A.'s in the managerial ranks.

These criticisms aside, this is a well-researched, well-written book. It deserves a careful reading by all specialists in the economic history of modern Europe and particularly by those interested in the issue of comparative national economic performance or the development of business theory and business education.

MICHAEL S. SMITH
University of South Carolina

KARL DIETRICH BRACHER. *The Age of Ideologies: A History of Political Thought in the Twentieth Century.* Translated by EWALD OSERS. New York: St. Martin's. 1984. Pp. xii, 305. $25.00.

Karl Dietrich Bracher, so Hans-Ulrich Wehler contended in 1979, "may well be the most significant West German historian of the past two decades." Works such as *Die Auflösung der Weimarer Republik* (1955), *Die nationalsozialistische Machtergreifung* (with Wolfgang Sauer and Gerhard Schultz [1960]), and *Die deutsche Diktatur* (1969) earned him this reputation because of their scrupulous attempt to detail the specific historical steps on the road to the German catastrophe. Rather than allowing us to remain content with overly ambitious pseudo-explanations of this most ineffable of events, he forced us to attend to the contingent circumstances that produced an outcome that was never foreordained while compelling us to take seriously the political and ideological forces that worked to bring it about.

In addition to being a master historian, Bracher has also taught political science at the University of Bonn and maintained a lively interest in political theory. It is essentially wearing this second cap that he wrote his most recent book, *Zeit der Ideologien*, which has been published in translation by St. Martin's Press. For, rather than providing a closely

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argued, meticulously researched account of a specific historical episode, it ranges over the political thought of the entire century and offers a global perspective on its subject. Bracher, moreover, has left behind the patina of historical neutrality that characterized his earlier work and now speaks with the voice of a politically committed observer deeply anxious about the potential he sees in the present, especially in his native country, for a repetition of the past. *Die deutsche Diktatur* ended with the confident slogan "Bonn is not Weimar"; now fifteen or so years later, he seems less certain.

The vantage point from which Bracher speaks is perhaps best called neoconservative, despite his specific qualms about the usefulness of the term. Throughout the book, his major preoccupation is to defend the totalitarianism analysis called into question by the radical historiography of the 1960s with its penchant for fascism as a generic category distinguishing Right from Left. Rather than stressing what might be called the structural roots of totalitarianism in Germany or elsewhere, Bracher emphasizes instead what he terms "ideologization," which he contrasts with the sober defense of liberal democratic ideas and institutions. He scorns attempts to see continuities between beleaguered liberal or centrist ideas and those of the radical Right in favor of the familiar argument that *les extrêmes se touchent*. And, like many neoconservatives who bizarrely see themselves as marginal outsiders in a world run by Reagan, Kohl, and Thatcher, he evinces an edgy pessimism about the future in an age still prone to radical ideologies and the totalitarian temptations to which they often lead.

However one may judge this analysis, what will make this book ultimately a disappointment to both supporters and foes alike is its paucity of fresh ideas. To anyone familiar with the work of J. L. Talmon, Eric Voegelin, Leszek Kolakowski, Karl Popper, and the group Bracher oddly calls "liberal sociologists" (Seymour Martin Lipset, Daniel Patrick Moynihan, Samuel Huntington, Daniel Bell, and Jeane Kirkpatrick), *Zeit der Ideologien* will contain no surprises. Along with its defense of Western democracy goes a predictable reliance on technological progress as an antidote to unrest. Thus, without pausing to consider the ideological assumptions in his own position, Bracher blithely asserts that "any lowering of performance or efficiency in favor of political participation, no matter how important or desirable, holds the risk of a serious upset of the equilibrium which can only just about be maintained, by further scientific progress and by relieving man of further burdens in the postindustrial society" (p. 229).

As a broad history of ideas, figures, and political events, this book rushes forward at too fast a pace to make an original contribution to any issue it treats. At times, Bracher stumbles in his attempts at rapid characterization, as when he describes George Orwell as a "former communist" (p. 197) and says Maurice Barrès "from the outset" wanted to harmonize nationalism and socialism, thus forgetting Barrès's radically individualist roots in a "*culte du moi*" (p. 58). But it is less these errors than the crabbed political vision of their author that ultimately rankles. Thus, the victims of the German *Berufsverbot* are snidely dismissed as "revolutionaries, as it were, claiming a pension" (p. 198), the crisis over the Vietnam War in America is called a "conflict between worldwide obligations and domestic self-doubt" (p. 204), and the hopeful policy of detente between East and West is dismissed as "certainly grounded also in ideology" (p. 196). The result is a kind of politically charged, woodenly translated, middle-brow handbook, which is as tediously predictable in its judgments as it is uninspired in its explanations. Those who share Bracher's avowed nostalgia for the 1950s, his favorite decade of the century, may find this book a comfort, but even they will find little new to fuel their "nonideological" view of the world. And those of us who learned so much from him as a historian will only regret his transformation into a political pamphleteer.

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RICHARD MARIUS. *Thomas More: A Biography*. New York: Alfred A. Knopf. 1984. Pp. xxiv, 562. $22.95.

This is by far the most comprehensive biography of Thomas More ever written, and it makes use of the large body of More materials that have been brought to light in the past two decades. As Richard Marius himself reminds us from time to time, his book is not hagiographical and thus differs from most More biographies and certain specialized studies such as J. A. Guy's on More's public career. There is great learning in the book, but the reader will have some difficulty evaluating the arguments because, for financial reasons, footnotes have been minimized wherever possible. Much of the time Marius is carrying on a debate with previous More scholars, but he presumes the reader already knows the position he is questioning, just as he assumes a thorough knowledge of the sources, since he does not attempt to document his every assertion.

Most readers will have points of disagreement with Marius's interpretations, but his arguments are generally plausible and faithful to the evidence. There is no doubt that the tapestry of More's life as here laid out is richer, more complex, and more intriguing than that found in traditional hagiographical biographies. But a blatant bias, which can only be called contentious, informs the work. I do not

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want to speculate about Marius's personal beliefs, but his pages reflect the perspective of the modern unbeliever who is made uncomfortable and even angry by the "fanaticism" of the sixteenth century. (The same viewpoint, possibly even more pronounced, is obvious in Marius's earlier work on Martin Luther.)

The bias is most obvious in the interpretive principle, stated explicitly on page x, that the fierce religious controversies of the sixteenth century masked gnawing doubts, on the part of both Catholics and Protestants, about the very existence of God. At best, this can only be surmised or even guessed, and modern psychology shows that this is scarcely the only possible explanation of "fanaticism": if people really believe that great truths are at stake, whether in religion or in politics, they are likely to become vehement about them. Marius's attempt to glimpse into More's soul is thus in the nature of things unproven and unprovable. Similarly, Marius speculates that an obsessive tension in More's life was caused by his decision to marry: More's sexual drives were too strong for him to remain celibate, yet he felt guilty for abandoning the higher calling of the monastic life. Evidence can be found for this interpretation, but the thesis still remains contentious. In too many places the author introduces a conclusion with "It is not perhaps too much to speculate. . . ."

Occasionally Marius's account is inaccurate. In the introduction, for example, John Henry Newman not only is incorrectly associated with Anglo-Catholic ritualism but also is made to seem an opponent of Darwinianism; in fact, Newman's few comments on Darwin seem accepting of that scientist's ideas. In another place papal infallibility is equated with the belief that priests cannot sin (p. 457), an equation that the Catholic church has been careful not to make. Most serious is the author's confusion of the Lutheran doctrine of predestination with the general Christian doctrine of divine providence (p. 472), a telescoping that renders the theological controversies of the sixteenth century virtually unintelligible. In addition, Marius is sometimes unable to control his polemical impulses, as when he describes medieval urges that made people "crawl dismally on all fours through the dark sewers of their hidden selves," a description that would surely apply more justly to modern psychotherapy (p. 320).

Given Marius's perspective, More's polemical exchanges with William Tyndale—probably the most neglected of all his writings in modern times—inevitably seem symptomatic of More's unbalanced mind. The dispute was indeed ferocious, but it is wrong to imply, as Marius does, that no intellectual substance underlay those books. In fact, they contain a sophisticated argument, made at the beginning of Western modernity, about the competing claims of individual and community, tradition and innovation, even (in the manner of Marshall McLuhan) about oral and print cultures.

Among his achievements, Marius has forever deprived modern "liberal" Catholics of one of their favorite saints. It was always something of an anomaly that they claimed him, and Marius correctly points out what has been forgotten only by those who wanted to forget it: that More was devoted unshakably to the authority of the Catholic church and countenanced the persecution of heresy.

As to the rest, the very obviousness of the author's biases help give the book enduring value. Had he been less blunt, his more dubious arguments might have subtly undermined the whole. As it is, the reader knows precisely where the author stands and can make the requisite allowances. The book is a kind of masterpiece and will receive the ultimate compliment of being used for years even by those who disagree with the author's entire point of view.

JAMES HITCHCOCK
St. Louis University

JAY PASCAL ANGLIN. *The Third University: A Survey of Schools and Schoolmasters in the Elizabethan Diocese of London*. Norwood, Pa.: Norwood. 1985. Pp. xi, 255. $29.50.

In 1612, George Buck wrote an enthusiastic account of educational facilities in the city of London. You could, he claimed, learn theology, grammar, law, sciences, languages, courtly skills, and sports to such an extent that London might be called "the third university" of England, after Oxford and Cambridge. Jay Pascal Anglin's use of this title for his book is not a happy one, since he disclaims reworking the whole of this field and limits himself to grammar, languages, and courtly skills. This occupies about half of the book; the rest is a rather different study of the schools and schoolmasters of the diocese of London (including Essex) under Elizabeth I. That too is not complete, for it omits the two important topics of the grammar curriculum and the pupils to whom schools catered. The resulting book is a series of articles rather than an integrated whole, and it would have been better to publish them in periodicals.

How then do the separate articles rate? The first, on pre-Elizabethan education in London, is confusing. Anglin has done little original work on this subject and draws mainly on secondary works. The topic is complex, and its reconstruction is not achieved here. The second, on Elizabethan endowed schools, is a useful but indigestible list of foundations in the manner of W. K. Jordan and ends with some good observations on the role of

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school governors. The third, on private venture schools, is based on archival research and is valuable, although one doubts the author's rigid categorization: there were fee-paying schools with some continuity and public status that were neither fully endowed nor fully private ventures. The fourth and fifth chapters, on schools of languages, fencing, music, and dance, are very interesting for historians of sport as well as of education, but more could have been said about the curriculum of music and the dance. The final study, of Elizabethan schoolmasters, is based on prosopographical research but fails to identify some major questions. It would have profited from use of work already published on these schoolmasters' late medieval predecessors. What was their standard of living, as shown by their material possessions? How well did they prosper if they left teaching for other work? And what do these factors, and other evidence, suggest about their status in society? There is more about schoolmasters in Elizabethan than in earlier literature, and it is possible to reconstruct the public view of them.

The book in general grates in other ways. There are an undue number of misprints and an abominably reproduced eighteenth-century map, which is the sole guide to a quite detailed discussion of school locations. Some relevant modern work has not been used. It is incorrect of the author to charge another historian with ignoring the educational role of parish clerks (p. 228), when ten years ago that person published a study of education in the west of England in which such clerks are mentioned. One must conclude, regretfully, that, although Buck's topics and the topic of the London schools are good ones, they have not been sufficiently realized here, and the book itself has been published far too quickly.

NICHOLAS ORME
University of Exeter

LAURA CAROLINE STEVENSON. *Praise and Paradox: Merchants and Craftsmen in Elizabethan Popular Literature*. (Past and Present Publications.) New York: Cambridge University Press. 1984. Pp. xiii, 252. $34.50.

Many years ago J. H. Hexter debunked the myth of the middle class in Tudor England. Since then, most historians have abandoned serious inquiry into the question of the nature and importance of the bourgeoisie and its values in sixteenth-century English society. Laura Caroline Stevenson offers an interesting, thoughtful, and new perspective on the Elizabethan middle class. She covers some of the same ground as Louis B. Wright's *Middle-Class Culture in Elizabethan England* (1935) but takes quite a different approach. Rather than simply accepting that popular Elizabethan fiction and drama is an expression of middle-class culture, Stevenson asks what the literature can tell us about sixteenth-century attitudes toward business and businessmen.

In examining the reflections of Elizabethan popular writers on the place of merchants and craftsmen within their society, Stevenson finds few unqualified examples of those middle-class values that can be summed up in the Weberian phrase, "the spirit of capitalism." Elizabethan popular literature celebrated the achievements of merchants and craftsmen in terms drawn from an older system of aristocratic values; she discovers chivalric merchants, gentle craftsmen, and lordly clothiers whose display of wealth and martial vigor are the equal of the nobility and the gentry. According to Stevenson, although merchants and craftsmen had more significant roles in English economic and social life during the Elizabethan period than in the past, the vocabulary available to authors was archaic and inadequate for the task of describing the nature of the social changes taking place. Although some Elizabethan writers such as Thomas Deloney came close to cre-ating an appropriate vocabulary, the task, in Stevenson's view, was not finally completed until the early eighteenth century, when authors such as Daniel Defoe and Richard Steele "discussed merchants and 'complete tradesmen' in terms of bourgeois values and gentlemen in terms of aristocratic values" (p. 7).

There is much to admire in this study, including Stevenson's careful reading of a wide range of texts and her general willingness to regard literature as worthy of serious consideration by the historian. Her arguments are enhanced by an awareness of recent work on Tudor social and economic history and by a readiness to view the literature in the context of that history. As a result, her analysis is considerably more sober and convincing than that of an older generation of scholars who approached the subject dazzled by glorious visions of Elizabethan colonial and mercantile expansion and the accompanying upward march of the middle class.

A few critical points need to be made. Stevenson draws too sharp a distinction between medieval and Elizabethan attitudes. In works of John Stow, Richard Johnson, and Thomas Heywood the principal London merchants are presented as more honorable and valiant in defense of sovereign and kingdom than the titled aristocracy. This can be regarded not as novel sixteenth-century praise for merchants but as a variation on a longstanding popular attitude expressed in social protests from as early as the mid-fourteenth century. On the occasion of riots or insurrections the failures of aristocrats and high-ranking officials to live up to their social and political obligations were contrasted, through parodies of existing legal and social arrangements, with the nobility of commoners. In her

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treatment of Deloney's *The Gentle Craft* (1597–98) Stevenson is too quick to identify as the spirit of capitalism a desire for gain that has all the hallmarks of the sin of covetousness known to medieval moralists.

At the other end of the chronological spectrum, Stevenson frequently asserts that, at the beginning of the eighteenth century, writers began to distinguish the middle class from the aristocracy by means of a set of entrepreneurial values. Yet this assertion is not sustained by the evidence she offers. On page 155, for example, she quotes a passage from Defoe as an example of the real "spirit of capitalism." The passage, in fact, provides a splendid illustration of the continuing inability of the English middle class to create a set of values distinct from those of the dominant nobility and gentry. The quotation is further confirmation of Hexter's point that membership in the landed elite represented the ultimate social ambition for the upwardly mobile members of the middle class.

Nonetheless, I do not want to conclude this review on a critical note. I found the book an engaging and perceptive work that has important things to say about both Elizabethan social history and the connection between literature and society.

BUCHANAN SHARP
University of California, Santa Cruz

PAUL S. SEAVER. *Wallington's World: A Puritan Artisan in Seventeenth-Century London*. Stanford: Stanford University Press. 1985. Pp. ix, 258. $29.50.

In *Wallington's World* a Stanford professor and a seventeenth-century London artisan have collaborated to produce a book that conveys the flavor of popular Puritanism with a vivacity scarcely matched outside the pages of John Bunyan. Nehemiah Wallington, a godly and not very prosperous turner of bowls and stools, lived from 1598 to 1658. He was remarkable—indeed, unique—in only one spectacular respect: between 1611 and his death he filled twenty thousand manuscript pages, of which twenty-six hundred have survived, with journals, letters, religious resolutions and meditations, sermon extracts, and political observations. Paul S. Seaver has assimilated this mass of material and employed it, together with some scant ancillary evidence and a great deal of sympathy and intuition, to re-create not only Wallington's family life and social milieu but also his mental landscape and his experience of the political catastrophe through which he lived.

After more than a decade of revisionist eclipse, the concept of Puritanism, suitably qualified and refined, seems at least to be coming back into academic fashion. *Wallington's World* will promote this wholesome trend. Seaver shows, beyond reasonable doubt, that an identifiable Puritan community did indeed exist in early Stuart England, although its members preferred to be called the children of God. "Puritan distinctiveness," says Seaver, "was obviously known and recognized both by other Puritans and by their non-Puritan neighbors" (p. 190). Wallington was born into this community, which Seaver suggests took shape in London during the 1580s. When sexual desire drove the young Wallington to despair of his salvation, he was deterred from suicide by the knowledge that "it would cause the wicked to speak ill of our profession, for thus they would say, 'look on these Puritans, see Master Wallington's son has killed himself'" (p. 22).

This godly community extended across social frontiers, linking "artisans and husbandmen, ministers and gentlemen." The godly believed themselves obliged to impose their rigorous moral standards on society at large. Puritanism, as Seaver observes, was "an active, potentially revolutionary movement" (p. 194) even before the rise of Arminianism—however earnestly Puritans professed their obedience to the crown. As Elizabeth, James, and Charles all perceived, their obedience was, in fact, conditional. The apparent apostasy of the Caroline regime in the 1630s, culminating in the king's supposed complicity in the Irish rebellion, turned men like Wallington into conscious revolutionaries by early 1642.

This book thus elucidates the origins of the English Revolution, but it does more. Seaver brings Wallington himself—"that lonely, God-struck artisan" (p. 39)—vividly to life. We see him chewing peppercorns to keep himself awake during the sermons to which he was addicted, operating his "mad calculus" (p. 32) of self-imposed fines for various sins, agonizing over real or anticipated domestic disasters. Wallington emerges, for all his anxiety and morbid introspection, as an oddly endearing character. Seaver's psychology is shrewd but unobtrusive, blessedly free of jargon and redundant theorizing. The style is rarely arresting but often achieves a restrained eloquence. Seaver describes, for instance, how Wallington, too old to fight in the Civil War himself, "sent his apprentice off . . . with as much enthusiasm as a humane and timorous man could summon" (p. 192).

The book also makes a notable contribution to the history of the family. Seaver shows that in Wallington's world "each new child was an endless source of anxiety, a being to be cherished and prayed over, and each death, however expected, as a crushing blow." On such occasions, "Nehemiah always found it difficult to summon the theologically appropriate feelings" (pp. 87–88). So much for the "affectionless" plebeian family imagined by some modern historians.

The book has its flaws. Seaver has not always managed the difficult feat of reconciling narrative

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and thematic sequences, so there is some ungainly repetition. The prose could be boiled down by a few percent without loss of flavor or nutriment. Gustavus Adolphus is indexed under "Adolphus, Gustavus." But *Wallington's World* is a triumph. Moreover, it is superbly appropriate for classroom use. It is to be hoped that a paperback is imminent, which will include a genealogical chart of Wallington's family, a chronological table, and a map of Wallington's London, while putting Gustavus Adolphus where he belongs.

WILLIAM HUNT
St. Lawrence University

RICHARD MIDDLETON. *The Bells of Victory: The Pitt-Newcastle Ministry and the Conduct of the Seven Years' War, 1757–1762*. New York: Cambridge University Press. 1985. Pp. xiii, 251. $44.50.

Julian Corbett's famous *England and the Seven Years' War* (1907) and Basil Williams's *William Pitt* (1913) established the conventions for understanding how king and ministers conducted the war of 1756–63. Corbett called the imperial conflict "Pitt's war" and saw "Pitt's system" as the key to England's victory. Williams gave Pitt heroic qualities to which Winston Churchill subsequently has been favorably compared.

Richard Middleton's study is the most extensive critique in print of the Corbett-Williams orthodoxy. In building his revisionist case, Middleton rejects the truisms that historians have taken from John Almon's unreliable *Anecdotes of the Life of the Right Honourable William Pitt* (1795) and reconstructs the interaction between king and ministers from an impressive array of primary sources found in British and American manuscript collections. His thesis is that England's victories came from the ministry's luck, competence, and persistence rather than from Pitt's domineering personality and genius. Even the duke of Newcastle, whose tarnished image as a fussy manipulator of patronage not even Reed Browning's 1975 biography could improve, appears in a favorable light. Middleton credits him with successfully financing the war and with adhering to principles (Lord Shelburne thought he had "no resolution or mind of his own"), which can especially be seen in his partiality to peace.

Middleton's heterodox interpretation of Pitt is more complex. The main point is that Pitt was not the author of a grand strategic plan for victory, because he had a deficient understanding of military problems in North America and because strong departments in the government kept him from being "solely responsible for the planning and conduct of the war" (p. 49). Middleton's secondary points are that Pitt easily supported the war in Germany that he had roundly criticized while in opposition, that he did not intimidate the generals, admirals, or the king's ministers, and that he did not advance young, able officers over stodgy senior officers in the fighting services. Middleton deflates Williams's proud, confident Pitt of 1756, who proclaimed that he alone could save the country from military disaster—a statement of doubtful veracity, argues Middleton, because the source was Horace Walpole's *Memoirs of the Last Ten Years of the Reign of George II* (1845), which "perpetuated a masterly fraud on the Whigs" (p. 224). Outside the House of Commons the new Pitt showed deference to king and colleagues and found his neurotic energy held in check by constitutional restraints. As Middleton makes clear, the king was England's executive officer and his servants carried out "his pleasure and the drafting of his commands" (p. 19). As secretary of state, Pitt was a "channel of communication, rather than a source of all activity" (p. 23).

This book, essentially an interpretive essay of 232 pages exclusive of bibliography and index, should not be compared with Corbett's almost 900–page work or the long narrative in the pertinent volumes of Lawrence Henry Gipson's study of the British empire. Yet four caveats need mentioning. First, Middleton has a tendency to overstatement. The assertion that under Newcastle's direction "the war had been fought and paid for" (p. 213) ignores the substantial war debt that existed in 1763. Second, the sources for the raids on the French coast (especially the Shelburne Papers at the Clements Library) suggest an active Pitt who was more than a "channel of communication." Third, considering the author's stress on using the best sources, one is surprised that he relied on the Toynbee edition of Walpole's correspondence rather than on the current W. S. Lewis edition. Last, I believe that Middleton's assertion about the uniqueness of his study's place in the literature is misleading. As early as 1955 the late Eric McDermott suggested lines of investigation that Middleton and I have pursued independently for two decades. These caveats aside, *The Bells of Victory* is a significant essay in revision that deserves careful reading and should stimulate serious debate concerning the role of Pitt and Newcastle in the conduct of the war.

WM. KENT HACKMANN
University of Idaho

B. W. HILL. *British Parliamentary Parties, 1742–1832: From the Fall of Walpole to the First Reform Act*. Boston: Allen and Unwin. 1985. Pp. x, 272. Cloth $28.00, paper $9.50.

The debate on party in eighteenth-century England is a good example of the ebb and flow of historical controversy. Before Sir Lewis Namier, the orthodox

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view presumed the continued existence of Whigs and Tories from the days of Clarendon to those of Gladstone. Namier waged a highly successful war against that orthodoxy, insisting that the 1750s and 1760s could be explained without reference to party at all. The support he received from other scholars (notably, Robert Walcott) led him to extend the argument to the rest of the eighteenth century, seeing factions, groupings, and shifting alliances as the norm. But from the 1960s onward a counterattack developed. Specialist historians reiterated the crucial importance of party in their chosen fields. Anne's reign was reclaimed for party by Geoffrey Holmes and William Speck, mid-century by Linda Colley, and the later eighteenth century by Frank O'Gorman and Donald Ginter. Namier's nonparty period looked more and more like a small sandbank surrounded by choppy seas.

Now even the existence of that sandbank seems threatened. Namier's argument was based on the assumption that the Tories disintegrated in the 1760s, leaving only spurious continuity between the parties in Walpole's day and those in Pitt's and Fox's. Certainly crossing over occurred in the early years of George III's reign. The Tories, after forty years in the political wilderness, moved over to support government, and Newcastle's Old Corps Whigs found themselves in unfamiliar opposition. B. W. Hill appeals from early Namier to late Namier, in the form of the *History of Parliament* (1964), to demonstrate a considerable cohesiveness and continuity in Whig and Tory personnel.

This volume is a very useful addition to Hill's previous work, which traced party development up to the fall of Walpole. It is helpful to have a debate, which has become rather ragged, tidied up. But the need to cover so long a period means that, at times, the theme is buried under straight political narrative. The extent to which narrative can illuminate the problem is limited, and one would have welcomed more attention to party organization and management and more discussion of recent writings on the subject, some of which challenge Hill's conclusions. There is little doubt of the continued existence not, indeed, of a party system but of coherent and identifiable parties during the century. What is less clear is how deep party divisions ran. They certainly did not prevent the emergence of a considerable consensus, for example, on the beauties of the constitution. An emerging issue seems to be the relationship between party and the aristocratic-gentry oligarchy. Provided that party differences were not too deep, they may even have promoted stability by affording the appearance of profound disagreement without the unpleasant reality. There could be life in the controversy yet.

JOHN CANNON
University of Newcastle upon Tyne

K. D. M. SNELL. *Annals of the Labouring Poor: Social Change and Agrarian England, 1660–1900.* (Cambridge Studies in Population, Economy, and Society in Past Time, number 2.) New York: Cambridge University Press. 1985. Pp. x, 464. $49.50.

In this masterly work K. D. M. Snell opens so many windows into the lives of the "labouring poor" that for the first time we can appreciate fully the extent and nature of the social crisis that overtook the southern counties of rural England in the years from 1790 to the 1840s. It is entirely appropriate that this region and this period receive Snell's closest attention, despite the book's more inclusive title. For it was there and then that a large proportion of the population, comprised of both agricultural laborers and rural artisans, suffered a searing and multifaceted decline in their standards of living. Snell is very much aware that the issues involved in charting such a decline are notoriously complex and treacherous, and, perhaps because of this, his treatment of those issues rises to a level of sophistication that is uniquely satisfying. In particular, he labors to avoid current notions concerning conditions of poverty and instead adopts what would have been the perspective of the eighteenth- and nineteenth-century poor themselves. This in turn leads him to conceive of his subject in the broadest sense and to include the more elusive, qualitative aspects of rural life.

Even so, his conclusions are solidly grounded on the most comprehensive and revealing quantitative evidence ever compiled for a study of England's rural population in these years. In what must have been a protracted and painstaking project, the bulk of this evidence was gleaned from a multitude of official examinations of impoverished country folk to determine their legal place of poor law settlement. In addition to demonstrating the richness of the settlement records, the range and precision of the data derived from them are a tribute to the imagination and verve with which the author handles these underused sources.

But it must be said that not even Snell can entirely overcome the inevitable weaknesses and gaps in materials of this kind. Although arguments resting on a leap of faith are generally kept to a minimum, his statistics on farm wages may seem to some to rely too heavily on certain unwarranted assumptions. Because his wage figures only reflect changing conditions for unmarried persons hired by the year, inclusive of lodging and board, he asks us to accept, in order to broaden the relevance of his data, that married persons hired solely at wages for short periods of time shared similar experiences. Whether one is prepared to accept such a view is of some significance, since many of his arguments ultimately depend on the applicability of his statistics on wages. Likewise, his evidence and conclusions fail to ad-

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dress the question of underemployment, an important factor in determining conditions for workers in arable agriculture. This is especially true for an assessment of the years from 1795 to 1834 when the poor laws were often geared to disguise unemployment through such mechanisms as labor rates, allowances, and the roundsman system.

These difficulties notwithstanding, Snell's meticulous documentation and forceful presentation leave no doubt that life for the nameless subjects of his book was grim and getting grimmer after 1780. The catalogue of transformations that he elucidates is extensive and nothing less than tragic: the loss of commons rights and the use of waste lands, the intensification of seasonal unemployment, the decline of real wages (at least for unmarried workers), the increasing exclusion of women from the agricultural labor market, the erosion of "farm service" and apprenticeship along with their varied protections, the decay of alternative employments in cottage industries, the proliferation of obstacles to geographical mobility, the ravages of the New Poor Law on public relief, and the rising tide of confrontations between the poor and those who controlled access to employment, wages, and relief. All this, Snell makes clear, represented the collapse of a whole way of life rather than a simple decline in material conditions. At heart, the historical process that he reveals for us with great skill and insight entailed the supersession of an older "moral economy" by the greater competition of spreading market relations. It is noteworthy that this process occurred earliest and most calamitously in what Snell characterizes as a "de-industrializing" region. The longer survival of a host of rural traditions and protections for agricultural laborers in the northern counties—the heartland of industrial England—should give pause to those who might be tempted to regard advancing industrialization as the driving force of agrarian capitalism.

Snell promises that country life in these same northern counties will be the subject of his next book. If that project even approaches the quality of the study under review here, he will have succeeded in producing, for the whole of rural England, the seminal works on the "labouring poor" in the eighteenth and nineteenth centuries.

PETER DUNKLEY
Georgetown University

JOHN R. GILLIS. *For Better, For Worse: British Marriages, 1600 to the Present*. New York: Oxford University Press. 1985. Pp. x, 417. $29.95.

ALLEN HORSTMAN. *Victorian Divorce*. New York: St. Martin's. 1985. Pp. 196. $25.00.

"The divisions of modern England originated in the history of Respectability more than in class struggle" writes Allen Horstman in the conclusion to his breezy account of the 1857 Divorce Act (p. 171). The thesis of his book is woven around a series of (sometimes careless) summaries of louche divorce proceedings taken from official *Law Reports*. He posits that the act was passed and a special divorce court established, because the fear of exposure and publicity surrounding divorce would deter the "sinful" adulteries that were the only allowable cause in an English divorce until the mid-twentieth century. Of all commandments, the seventh was most influential, even on a society that was loosening its religious bonds. "Appearance overshadowed substance" (p. 161).

The divorce rate increased appreciably immediately after the act had been passed: individual parliamentiary proceedings were no longer required for each case, and the middle class could now afford divorce. Nevertheless, the double standard allowed men to divorce their wives solely for adultery, whereas women had the far more difficult task of proving adultery, cruelty, and desertion. Both this and women's economic dependence kept the number of English divorces throughout the Victorian period to a minimum. Moreover, the opprobrium following divorce court publicity was so great that no woman involved in such a proceeding could remain a respectable member of society. In Horstman's view, the divorce court kept women chaste, forced them to condone their husbands' extramarital affairs, and stablized the respectable Victorian middle-class family.

Horstman, a lawyer interested in history, deals almost exclusively with divorce among the Victorian middle and upper classes; John R. Gillis, a historian interested in anthropology, deals with the heterosexual living arrangements of those below the middle class. He argues that the conjugal ideal has always been too fragile to withstand the reality of marriage. From the sixteenth century, the majority of both men and women, but particularly women, have depended emotionally and economically on their families and friends rather than on their spouses. In deliberate contrast to Lawrence Stone and Randolf Trumbach, Gillis argues that companionate and egalitarian marriage did not develop among peasants, paupers, and skilled or unskilled workers. He discusses the symbolism of betrothal and marriage rites, clandestine and common-law marriage, and wife selling as the poor couple's means of coping with the problem of an indissoluable marital bond. For almost the entire period covered, the cost of legal divorce was prohibitive for the working class. For them marriage was, despite the formalities of the law, irrevocable.

The lack of finality offered by the preindustrial custom of "betrothal," however, gave couples the ability to change their minds, to be together, to

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indulge in intimacy and sexual intercourse, and to use the occurrence of pregnancy as the basis for marriage. Marriage was a political event; it meant power and status for the husband, who thus achieved authority not only within the family but also in the community. Large weddings produced the necessities for setting up house. As late as the 1850s, respectable people of slender means used "bidding weddings" to collect gifts of all their basic households goods from neighbors. Such wedding rituals were a buttress for the frailty of conjugality and highlighted the couple's dependence on the community. As the "bidder," who invited the wedding guests, put it, "A great many can help one, but one cannot help a great many" (p. 153).

In preindustrial rural society, as well as in the protoindustrial and early industrial periods, lower-class couples were united as late as possible. London became a "celibate city" owing to the late marriage or nonmarriages of the men and women who could afford neither the price of the wedding license nor the cost of survival without the extended family, which remained in the countryside. Gillis comments that both common-law marriage and public wife sales reflect social responsibilities rather than a victory of individualism (p. 218–19). Even so, much as we may admire a society that accepts collective responsibility for its members, a wife sold at cattle market with a halter around her neck, offers a painful symbol of how that society viewed the relationships between men and women, husbands and wives.

By the mid-twentieth century, British working-class conjugal relationships were as fragile as ever. Gillis's analysis of the separateness of the young of both sexes and the ignorance they brought to their marriages underscores the existence of similar conditions for the middle and upper classes. As upper-class husbands escaped to their clubs, so working-class husbands retreated to the pub and the football match, while their wives clustered around their own mothers and women friends.

Gillis's thesis makes good sense; his evidence is original, comprehensive, and persuasive. The book is a noble attempt to produce an all-encompassing history of lower-class marital arrangements and to deal with the intricacies of regional and distinct class variations. Unfortunately, this leaves even the experienced reader lost in the maze of British class distinctions and disoriented by rapid moves from preindustrial through protoindustrial to industrial society, from peasant to rural or urban pauper, from north to south, from England to Wales. This complexity is a problem for all social historians and their readers. It has yet to be solved satisfactorily.

SUSAN GROAG BELL
Stanford University

PETER GLASSMAN. *J. S. Mill: The Evolution of a Genius.* Gainesville: University of Florida Press. 1985. Pp. xvi, 188. $18.00.

Peter Glassman's book is a psychoanalytic approach to John Stuart Mill's life as well as an effort to explain Mill's writings as a reflection of his early traumas. Mill is portrayed as "a hero of the human spirit" because of his remarkable resilience in the face of a severely repressive childhood (p. 168). The result is uneven at best: although provided with occasional illuminating insights, the reader is burdened by the jargon and reductiveness of the author's method, the repetitiveness of his principal ideas, and the impossibility of verifying any of his conclusions.

Mill, we are told, was the son of an omnipotent father and of a woman submissive as a wife and deficient as a mother. James Mill undertook to make John his intellectual companion and saw to it that the son despised his mother. Yet James continued to sleep with his wife and fathered nine children, eight after John's birth. From these premises Glassman argues that the son suffered from his failure to possess his father completely. The younger Mill became the victim of an inverse or negative Oedipus complex, the author assures us, and wished to become his father's spouse; we are told elsewhere, in a somewhat contradictory mode, that Mill saw himself as "his father's symbolic phallus" (p. 34).

We are also told that the charm of Harriet Taylor, with whom Mill fell in love and whose intellectual talents he much exaggerated, was in Mill's having identified her with his father. Glassman argues that Mill constructed an identity built on his "emasculated role as his father's and his wife's psychological vassal," which was a "more satisfying" situation than the only available alternative, that of possessing "no identity at all" (p. 160). This situation was an "odd, exteriorized narcissism" (p. 161).

Even more off-putting is Glassman's discussion of Mill's principal writings as attempts to overcome his neurosis. Nowhere does the author associate Mill's views with real political or social issues, nor does he set them in the context of general ideas. *On Liberty*, for example, is seen merely as the work of a "radically repressed" man whose father had required him "to fear and to despise liberty." To write it, Mill had "to reinvent his imagination" and "to change everything he ever before had believed about order, duty, purpose, love" (p. 119). Glassman similarly disposes of Mill's other writings.

All is made subservient to Mill's neurosis, constructed by Glassman in part by ignoring evidence that other psychoanalytic historians such as Bruce Mazlish and Peter Gay have found useful. For example, he does not mention Mill's dream of the sincere Magdalen, described in an 1857 letter to

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Harriet, whose import cannot be easily adapted to the author's position. Consequently, despite a fluent style and some useful perceptions, even a reader not unsympathetic to the possibilities of a Freudian approach remains unpersuaded.

BERNARD SEMMEL
State University of New York, Stony Brook

NOEL ANNAN. *Leslie Stephen: The Godless Victorian.* New York: Random House. 1984. Pp. xv, 432. $25.00.

In 1951, Noel Annan published what became the most important work on Leslie Stephen, more a critical study than a biography. In 1984, Lord Annan, newly retired from the vice-chancellorship of the University of London, went over the same ground in a larger compass and with much fresh material. The present work is at once new and not new: it is revised, recast, and enlarged but reuses much of the original text. The biographical introduction, enriched by new sources, is concentrated in the first five chapters. The rest is history of ideas in the grand style, taking notice of recent scholarship (largely American) and of a further generation of intellectual debate, in which Annan, man of letters as much as historian, feels free to engage (note the critique of psychohistory on pages 137–40). This 1984 avatar deserves to be reviewed as if it were a new book, both because of its new material and because of its significance for intellectual history.

Stephen is remembered as the quintessential Victorian man of letters, the first editor of the *Dictionary of National Biography*, and the father of Virginia Woolf (of whom, for the sake of his reputation, he should have been afraid). A member of the "intellectual aristocracy" (which Annan described in a classic 1955 article) and the descendant of noted evangelicals, Stephen lost his faith, left the church and the university in which he held office, and became a rationalist and agnostic. This is the theme that Annan chooses to emphasize in his new edition, as the subtitle suggests; nonetheless, the book recognizes Stephen's pioneering work in the history of ideas and retains the structure of a general study of thought in historical context indicated by the 1951 subtitle, *His Thought and Character in Relation to His Time*. Biography is subordinated to intellectual history because "his writings are of greater interest than his life and that to understand Stephen is to understand Evangelical morality and Victorian rationalism" (p. xii). Stephen (and Annan with him) regarded his unique achievement as the creation of a rationale for unbelief that retained an imperative morality, substituting ethics for theology and biography for casuistry. Stephen had kept evangelical morality even when he no longer could keep the faith. Although Annan rightly criticizes Stephen's "scientific" ethics, a compound of utilitarianism and Darwinism, he credits Stephen with justifying the divorce of faith and reason by successfully separating ethics from religion. If, in agnostic eschatology, biography is equivalent to purgatory, Annan has been reasonably liberal with his indulgences, and he has restored Stephen to his rightful place as an eminent Victorian.

The volume is illustrated and includes some of Stephen's own charming sketches. Reviewers rarely comment on book jackets, but Loretta Leiva's design is so elegant as to call for praise.

JOSEF L. ALTHOLZ
University of Minnesota

VIVIAN GREEN. *Love in a Cool Climate: The Letters of Mark Pattison and Meta Bradley, 1879–1884.* New York: Clarendon Press of Oxford University Press. 1985. Pp. xvii, 269. $29.95.

Love in a Cool Climate is a significant contribution to our understanding of Victorian academic and family values. Based on some four hundred fifty letters between Oxford scholar Mark Pattison, an unhappily married man in his sixties, and Meta Bradley, an unmarried women forty years his junior, from 1879 to 1884, this book offers an intimate record of upper-middle-class Victorian life.

Pattison (1813–84), the rector of Lincoln College, Oxford, is known today as an early associate of the Tractarians, a rival of Benjamin Jowett in the field of university reform, and the probable prototype for George Eliot's Dr. Casaubon in *Middlemarch*. He was noted in his own time less for his substantive accomplishments than for his total dedication to a life of learning. His scholarly achievements included a biography of Isaac Casaubon (1875), a short life of John Milton (1879), and an unfinished history of European learning. His posthumous *Memoirs*, which traces the evolution of his theological and intellectual ideas, is a minor classic in English literature.

Pattison's personal life was bitterly unhappy. His election to the rectorship of Lincoln College in 1861 was immediately followed by his marriage to Emily Francis Strong, who was twenty-seven years younger than her husband. Resentful of the curbs marriage placed on her independence and of her husband's insistent demand of his conjugal rights, the young wife soon began to live part of the year in London, where she developed an intimate relationship with Liberal politician Sir Charles Dilke. In 1879–80, Pattison found consolation in a "strange and perfect friendship" (p. 172) with Meta Bradley, niece of the master of University College. During the fall of 1880, Bradley stayed at Pattison's Oxford lodgings, the only time in their four-year relationship that they lived under the same roof. The strong

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disapproval of Pattison's wife and Bradley's father prevented continued meetings or regular correspondence. Letter writing was renewed, however, after her father's death in March 1883 and continued until Pattison died in July 1884.

"Our letters," Bradley wrote to her correspondent in 1883, "would be invaluable in 1983 as shewing the life of this age" (p. 207). This is indeed the case. This volume of letters provides a remarkably moving portrait of people who had abandoned accepted conventions in private life but who still sought to sustain a respectable public image. The reader gains vivid glimpses of the Pattisons' repeated efforts to sustain the artifice that they were a happily married couple; of Emily Pattison's strenuous attempts to maintain the image of dutiful wife; and of Bradley's symbolic ways of demonstrating that she remained a loyal and deferential daughter.

The editor, Vivian Green, the current rector of Lincoln College, has meticulously arranged and edited the letters to read like a novel. Social and cultural historians alike will find these letters a mine of information about the awkward position of an unmarried Victorian daughter, the deep ambivalence with which many Victorian women approached marriage, patterns of filial and marital rebelliousness, and the mechanisms through which public opinion influenced private behavior. Although some of the editor's psychological assertions are open to question—such as his speculation that Bradley was "frigid" (p. 61)—the analysis is generally restrained, and the letters are allowed to speak for themselves.

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TROWBRIDGE H. FORD. *Albert Venn Dicey: The Man and His Times*. Chichester, England: Barry Rose. 1985. Pp. 354. $30.00.

WILFRID E. RUMBLE. *The Thought of John Austin: Jurisprudence, Colonial Reform, and the British Constitution*. Dover, N.H.: Athlone. 1985. Pp. 278. $40.00.

Wilfred E. Rumble and Trowbridge H. Ford are both political scientists attempting revisionist explanations of two nineteenth-century legal theorists. Rumble's judicious study argues that John Austin was important as a legal philosopher who contributed to utilitarian ethical theory by connecting utilitarian principles and moral rules. In a close textual analysis of Austin's writing, Rumble finds that his conceptions of law, sovereignty, jurisprudence, and normative and actual law were determined by utilitarian measures of general happiness. Ford's injudicious, confusing polemic, based on allegation instead of demonstrated evidence, concludes that a self-serving and deceitful A. V. Dicey should be remembered essentially as a creator of the evils that plagued Ireland in the twentieth century. Instead of assessing Dicey's work within a broader social, political, and intellectual context, Ford assigns Dicey a detailed background of influences, motives, and conduct that appears to be entirely conjectural.

Conceding that Austin was neither a first-rate nor even a seminal thinker, Rumble justifies an intellectual biography because Austin contributed to the development of legal positivism and jurisprudence. Since Austin's letters were destroyed during World War II and Rumble is no specialist in the early nineteenth-century period, he devotes only one of seven chapters to Austin's life and its relationship to his thought. The historical context is based on a haphazard selection of secondary works, mostly surveys, but Rumble reads Austin's works from an informed legal perspective that recognizes the issues affecting jurisprudence and the relations between law and public policy. Although Rumble accepts the influence of Hobbes, Locke, Malthus, Adam Smith, and Ricardo, he sees Jeremy Bentham as the most enduring force in Austin's intellectual development. Austin learned from Bentham a utilitarian calculus. He also learned dissatisfaction with English law, faith in legal certainty and codification, a rejection of theories rooted in natural and moral rights, a conception of law as command, a theory of sovereignty, and a sharp distinction between actual and ideal law. But, as his neighbor and mentor moved closer toward democracy, Austin grew increasingly conservative, neurasthenic, and difficult. From his initial failure as a practicing lawyer from 1818 to 1825, Austin declined through further failures as professor of jurisprudence and the law of nations at the University of London from 1829 to 1835, as a criminal law commissioner in 1833, and as a royal commissioner to Malta from 1836 to 1840. From 1835, Austin and his family lived largely abroad in France and Germany, returning to England only after the Continental revolutions of 1848–49, which reenforced his fears about democracy. Rumble is disappointed in Austin because he lacked the energy, will, and discipline of a John Stuart Mill and only twice in his life mastered his indolence to write his London lectures, *The Province of Jurisprudence* (1832), and his reports on Malta in the late 1830s.

A discussion of divine law, utilitarian ethics, and positivist jurisprudence roots Austin's ethical system in divine law, but conduct and the conformity to rules were tested by the principle of utility. Austin understood law as an enforcer of standards, but he also wanted a group of ethical experts to have the authority to recommend which rules would be followed. Among these rules Austin emphasized individual self-interest, and he defined the end of government as the greatest possible advance of individual and hedonistic goods. It is remarkable

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that so many nineteenth-century thinkers who promoted hedonistic theories had so little pleasure themselves. In Austin's case, the hedonistic emphasis was mitigated, as Rumble demonstrates, by his notion that law contained certain limiting ideas such as duty, right, liberty, injury, punishment, and redress and by his insistence on a legally unlimited sovereign who could invoke punitive sanctions against individuals. Austin was more original in his penetrating analysis of the law that judges actually make when they attempt to apply existing law on the basis of their own ideas of public policy or expediency. Austin advocated more candid judicial legislation, especially when established rules were too vague or inconsistent or when social, economic, or other changes required an adjustment of the law. Although Austin distrusted the uncertain legal effects of judicial legislation, he believed that in a mixed system of statutory and common law the benefits were greater than the costs.

Although interested essentially in Austin's effect on recent sociological and realistic philosophies of law, Rumble provides a detailed analysis of Austin's attempted reform of Malta and specifically his comprehensive law of criminal libel, enacted into law in 1839, within the context of early nineteenth-century utilitarian discussions of the freedom of political criticism. Then, turning to Austin's last published work, *A Plea for the Constitution* (1859), Rumble considers Austin's defense of autocratic power and his opposition to any reform of the constitution. In conclusion, he sees Austin as a clear and realistic writer committed to the good of society as he understood it. It is not clear that these qualities make Austin an important nineteenth-century thinker.

There is less doubt about Dicey's importance, particularly as a seminal interpreter of the relations between law and public opinion in the late Victorian period. But Ford is not interested in this Dicey. Beginning with a childhood in which his mother is assumed to have read certain books to Dicey that determined his future thought and conduct, Ford constructs a largely incoherent account of Dicey's life and conduct that includes the claim that "Dicey was apparently behind the plot against (Charles) Dilke" (p. 159). A more reliable guide to Dicey's thought and career can be found in Richard Cosgrove's *The Rule of Law: Albert Venn Dicey, Victorian Jurist* (1980).

REBA N. SOFFER
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GORDON MARTEL. *Imperial Diplomacy: Rosebery and the Failure of Foreign Policy*. Buffalo, N.Y.: McGill-Queen's University Press. 1986. Pp. xiii, 307. $37.50.

"Old-fashioned perspective" is the phrase Gordon Martel uses, somewhat defensively, to characterize his study of Rosebery's foreign policy (p. 253). But it is a perspective that is particularly apt for the subject, since Rosebery's attitude was always to formulate a policy that stood above pressure groups, sectional interests, even domestic consequences. His approach was bipartisan; his goal was the security, but not necessarily the extension, of the British empire. Rosebery has not always been seen in this light, but it is precisely Martel's aim to foster a new understanding of Rosebery, who has been too much studied from the standpoint of interparty relationships (notably, those with Gladstone and Harcourt) and too little from that specifically of foreign policy.

And it is true that Rosebery has been little studied. In part this is no doubt because of the relatively brief period in which he held power—he was foreign minister for six months in 1885 and for a year and a half in 1892–94, and he was prime minister for another fifteen months in 1894–95 (with Kimberley in the Foreign Office). During his tenure no major crises catch the eye; rather, Rosebery handled such matters as Uganda's status, Congo frontiers, Mekong villages, frontiers in the distant Pamirs, and the effects of the still more distant Sino-Japanese War. Finally, some of the ground has been covered, most notably by Robert Rhodes James in the standard biography and G. N. Sanderson's excellent study *England, Europe, and the Upper Nile, 1882–1899* (1965). James, however, could not give the same attention to the detailed intricacies of foreign affairs, and Sanderson wrote without benefit of the extensive Rosebery papers, which Martel has used to good advantage. In short, there is indeed a place for this study.

Fortunately, Martel's book is thorough, erudite, and well written. Not every reader will accept Martel's interpretations, particularly his attribution to Rosebery of more power in deciding overseas policy than is usually assumed. The primacy of the individual foreign secretary's role is not a popular approach today (hence, Martel's assertion that the book is "old-fashioned"). Nor will all agree with the assessment of Rosebery's overall "failure" indicated in the title. Rosebery failed only if it is conceded that he shared the late nineteeenth-century grand design (not his invention) of using the Triple Alliance to balance France and Russia but making no entangling commitment that might push Britain to the head of the line in resisting those two powers. Such splendid isolation was not long to be enjoyed. From the standpoint of "crisis management," however, it can be argued—and Martel does argue—that Rosebery was reasonably successful. Rosebery's real failure, after all, must still be found in his personality, at once arrogant and self-doubting, which ensured that he was never again in office after 1895,

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thus warranting the epitaph of "a man who was widely expected to accomplish much but who, in fact, accomplished practically nothing" (p. 159).

As is normally the case with a lengthy work of such detail, this book contains a few small errors: for example, Russian troops were increased in districts bordering on Austria, not on Russia (p. 97), and no diplomatic Hardinge was ever a viscount (p. 193). Readers who wish to follow the fine points of territorial negotiations will have to pull down the nearest atlas, for no maps are provided. The abbreviated index is little help, although the selected bibliography is useful. These are small criticisms, however, of a very valuable book, which must be of importance to any student of Rosebery and of power relationships in the 1880s and 1890s.

BRITON C. BUSCH
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RUDDOCK F. MACKAY. *Balfour: Intellectual Statesman.* New York: Oxford University Press. 1985. Pp. vi, 388. $29.95.

Arthur Balfour was one of the most important British leaders of the past three centuries. For almost sixty years after 1875, he stood at or near the highest political echelons. While a young M.P., he served as private secretary to his uncle, Lord Salisbury; he held minor posts in the 1880s; he was chief secretary for Ireland, leader of the Commons, and twice first lord of the Treasury in the late 1880s and 1890s. When Salisbury retired in 1902, Balfour became Conservative party leader and prime minister; he served as first lord of the Admiralty in 1915–16 and as foreign secretary in the latter half of World War I and the peace conference period; finally, in the 1920s, Balfour acted as lord president of the council under two prime ministers. Even when his party was out of power, Balfour was influential, sitting, for instance, on the Committee of Imperial Defence in 1914. But he was more than just a politician. Throughout his life he was an intellectual who wrote with distinction on a wide range of philosophical and political subjects, practiced the *bon mot*, and was the darling of successive generations of British thinkers in the late nineteenth and early twentieth centuries.

Ruddock F. Mackay's purpose is not to write another biography of the man; he admits that four exist already. Instead, he seeks to put Balfour in historical context by rationalizing the contributions of the statesman with those of the intellectual in order to "contribute to a surer understanding" of Balfour's accomplishments. To make his case, Mackay examines what he sees to be Balfour's as yet unstudied contributions to education and to foreign and defense policy, "the main areas of Balfour's work as a statesman," which gives us a nice balance between domestic and external concerns. Although much archival material relating to Balfour has already been used extensively by his biographers and those who have written specialized studies touching other aspects of his career, Mackay claims to have used these sources in tandem with a lode of material as yet unscrutinized: "the extensive body of evidence at the Public Record Office relating to defence policy from 1902 onwards to the end of Balfour's career, education policy, especially in 1901–02, and foreign affairs from 1902 onwards" (p. vi). This is a laudable goal, and the proposed archival base on which it is to be achieved is one of unsurpassed breadth and depth.

It is a pity that Mackay does not achieve his goal. Although he does provide additional insight into Balfour's ideas on education, foreign affairs, and defense, the translation of these ideas into policy, or at least their effect on policy, is another matter. This stems from the archival base Mackay uses: new sources are not used to any great extent. His analysis of Balfour's contributions to foreign and defense policy is a case in point. It is based on private manuscript collections, which have already been used extensively by other historians (for example, the papers of Joseph and Austen Chamberlain, Lord Esher, and Maurice Hankey), and on cabinet papers both from the cabinet as a whole and from the Committee of Imperial Defence, which is again a corpus of material used in a plethora of studies. But Mackay did not bother consulting the one set of Public Record Office materials that other historians have not scrutinized to any large extent. These are the departmental records of the Admiralty and the Foreign Office (notably, ADM 116 and FO 371 and 608); they cover more than four crucial years, when Balfour served in key ministerial foreign and defense policy positions: from the cabinet crisis of May 1915 to the conclusion of the Versailles and St. Germain treaties at Paris. This is a serious handicap given the author's professed intention to base this book on unscrutinized archival materials. Moreover, Mackay did not bother to look at the papers of Sir Edward Grey or Lord Curzon, Balfour's predecessor and successor at the Foreign Office. Balfour's opinions carried weight in the policies pursued by Grey and Curzon before, during, and after World War I. Anyone looking at Balfour's career will have to consult this book because of the few insights it does give. For other historians whose work only touches on Balfour, however, it can be safely avoided. This is unfortunate given Mackay's ambitious intentions.

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ALAN J. KIDD and K. W. ROBERTS, editors. *City, Class, and Culture: Studies of Social Policy and Cultural Production in Victorian Manchester.* Dover, N.H.: Manchester University Press. 1985. Pp. 280. $35.00.

Every capable historian knows that the term "the middle class" must be used carefully. The ten contributors to this collection of essays advance our efforts to understand "the middle class" by showing how that class perceived itself and was perceived by other classes in nineteenth-century Manchester, the world's first industrial city and, as the center of cotton manufacture, the bulwark of British economic supremacy in the Victorian era. The first section of the book analyzes middle-class policy toward the working class by examining topics such as law enforcement, poor relief, responses to Jewish immigration, and support of local societies dedicated to civic improvement. A second section defends the thesis that a fairly coherent ideology was visible in the art, literature, and journalism purchased by the middle class for itself or produced for the working class.

As editors Alan J. Kidd and K. W. Roberts point out in their introduction, the book makes extensive use of neo-Marxist theories offered a generation ago by E. P. Thompson and Raymond Williams. Predictably the results are mixed. The essays contain a certain amount of pseudo-profound jargon and an occasional heavy-handed insinuation that the middle class liked nothing better than to dupe the saintly poor. But most of the essays are written in a spirit of disinterested, self-critical inquiry, and the book as a whole is an important contribution to our understanding of the nineteenth century. Perhaps the strongest chapters are"'Healthy Reading': The Periodical Press in Late Victorian Manchester" by Margaret Beetham; the equally informative "Representations of the Manchester Working Class in Fiction, 1850–1900," by Trefor Thomas; and "The Anti-Semitism of Tolerance: Middle-Class Manchester and the Jews, 1870–1900," by Bill Williams.

The book concludes with a detailed, highly useful bibliography by Terry Wyke designed to help readers pursue the questions raised in this volume.

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DAVID GARLAND. *Punishment and Welfare: A History of Penal Strategies.* Brookfield, Vt.: Gower. 1985. Pp. x, 297. $41.95.

Criminologists rarely link their work to the origins and development of the British welfare state, despite the regular concurrence of legislation in the penal and social spheres. *Punishment and Welfare* attempts to repair this neglect by examining the relationship between the penal system and other social institutions from the 1890s to 1914, during which time the late Victorian system made way for the "modern penal complex." The result is a historically based, if overly abstract, study of how penal change comes about.

The first five chapters present a required but prolix preface to penal reform. A derivative account of the "classical" Victorian prison system, based on mass uniformity, and its "positivist" successor, based on individualization, is followed by an equally derivative account of the broader social transformation in the regulation of the poor. This transformation derived from the ideological crisis of the 1880s and 1890s, when economic liberalism was found wanting. In the penal sphere the crisis concerned the failure of imprisonment to rehabilitate criminals and led to the appointment of the Gladstone Committee on Prisons in 1895. David Garland pretends to have discovered the 1890s as a watershed decade for penal change, although he must know this is the accepted wisdom of British penologists, if not of Michel Foucault. A new strategy of social regulation gradually emerged. A number of reform "programs" went into its making, two of which (the criminological and the eugenic) were sources of a new "penality," based on the "scientific" investigation and correction of "criminal man."

The last three chapters examine how these programs were translated into practice. Official reports, administrative initiatives, and legislation all played their part, but Garland appends to this traditional pattern of reform the preliminary conflicts over such issues as "responsibility" and "determinism," in which theoretical consistency was sacrificed on the alter of political consensus. The author finally submits the speculative argument that the measures to deal with habitual criminals, inebriates, and the mentally deficient were a crucial reinforcement for the Liberal welfare reforms, constituting a substitute for the labor colonies that the social security program failed to attain and forming the strategic link between the social and penal legislation that established the modern "penal-welfare complex."

Garland's understanding of policy making is extremely sound. In place of the inexorable logic of disciplinary society in Foucault's *Surveiller et punir* (1975) he postulates "a fragmented series of responses to perceived problems, responses which were constructed and struggled over within the limits set by the political situation" (p. 65). Unfortunately, the author follows up this promising statement with an abstract analysis of "official discourses." The latter has its place, but it should not be undertaken at the expense of personalities and pressure groups, which rarely make an entrance, or of such statistical information as the number of

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offenders sentenced either to imprisonment or to preventive detention and inebriate reformatories. In addition, the argument that welfare and criminal policy are interlocked is not all that historically grounded. Few historians will be convinced, for example, that Herbert Gladstone, chairman of the Departmental Committee on Prisons (1895) and home secretary (1905–10), was charting the same course as Winston Churchill and David Lloyd George, the two main champions of the "new liberalism" and of state-sponsored measures of social welfare. Even so, historians will doubtless find the book a thoughtful and challenging introduction to the origins of modern criminal policy. Finally, a captious query: must every description come in alliterative triplicate—"directed, delimited and deliberate" (p. 208); "diluted, disguised or displaced" (p. 225)?

VICTOR BAILEY
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ROGER DAVIDSON. *Whitehall and the Labour Problem in Late-Victorian and Edwardian Britain: A Study in Official Statistics and Social Control.* Dover, N.H.: Croom Helm. 1985. Pp. ix, 293. £18.95

Over the past few years Roger Davidson has produced, both by himself and in collaboration with Rodney Lowe, a series of extremely interesting and important articles on those sections of the state bureaucracy dealing with labor. The present book adds substantial detail to the arguments put forward in those essays, and it offers as well a coherent narrative of the process by which the British government came to grips with the pressing need to know more and to do something about labor. Its primary focus is on the Labour Department of the Board of Trade, which during the First World War became the Ministry of Labour, but Davidson also deals, where relevant, with the Home Office and the Local Government Board. These have never been among the more glamorous or powerful agencies of the state, and the ministers who have headed them have typically not been the "heavy-weights" in the government, but their work has nonetheless been of considerable importance.

That importance derives from two related facts. First, these agencies and their actions have largely constituted the daily practical relationship between the state and ordinary workers in Britain. Since that relationship has tended, over the course of the twentieth century, to become more and more critical, so the actions of these departments, particularly the Ministry of Labour, have become increasingly central to the successful functioning of government. Second, the changing labor policies of successive governments have been affected, not merely by changing what these departments do or do not do but also by altering the status and structure of these departments within the overall structure of the state.

It is critical to keep the two aspects of the story together, and this is what Davidson admirably does in his history of government efforts to come to grips with the "labor problem" from the late 1880s through 1914. What Davidson shows clearly is that administrative innovation was a highly political process. Simply establishing an agency designed to collect labor statistics, it was felt, entailed a commitment to doing something about the problems of labor. As a result, the fledgling Labour Department had to fight from its very birth for funds adequate to undertake the job it was given. In this battle the department was regularly pitted against the Treasury, whose officials sensed early on that more money spent to document social problems would lead to demands for even more money to solve those problems. It was best, then, not to know and to save money at both ends.

The main value of Davidson's book, therefore, is that it demonstrates in detail the interaction between social reform and administrative structure; in so doing, it provides a useful case study through which to evaluate various arguments about the social bases of the state and the growth of state intervention. In the process, moreover, Davidson provides a great deal of background information, which should prove essential to those scholars who make regular use of official statistics on prices, unemployment, and strikes. In sum, the book is a nice addition to labor history and to the history of administration.

JAMES E. CRONIN
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PATRICIA PUGH. *Educate, Agitate, Organize: 100 Years of Fabian Socialism.* New York: Methuen. 1984. Pp. xiii, 330. $33.00.

Patricia Pugh's work is difficult to assess, for it is neither a history of the Fabians and Fabianism nor an anecdotal album spiced with photographs but an incongruous combination of the two. It is also not an introduction to the Fabian Society's history for the student of modern Britain, whether neophyte or not, and does not replace the older histories. Pugh bases her work largely on the Fabian archives at Oxford, where she spent an apprenticeship organizing and classifying them. The bibliography of secondary works is sparse, however, and she had made no use of the numerous, massive dissertations written about individual Fabians or groups of them and about their ideas, work, and influence. Her principle of selection appears to have been to use the archival material to the utmost and to avoid any-

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thing that has appeared in earlier histories, which she wants to "complement," not "supplant" (p. xi). The result is an interim report, a prolegomenon.

In keeping with the book's institutional focus, the reader is filled in on the vicissitudes of membership, funds, production of tracts and pamphlets, conferences, summer schools, and autumn lectures. Some of her most interesting passages describe how the society organized a succession of grand schemes of multifarious research, but we have little indication of the significance of the findings or the uses to which they were put. Her concentration on the society as an institution, detailing the influences on it more than those it exerted, convinces me that its history cannot be divorced from the story of Fabian thought, action, and influence.

Pugh does not enter into any historiographical debates, nor does she joust with Eric Hobsbawm over his "reconsideration" that the Fabian genius was for public relations and their claim of great influence is "largely mythological." Rather, we are given a serenely accurate exposition of names, dates, tracts, and a succession of crises. She is particularly perceptive in linking the quarrels that beset the Fabians, such as over the policy of "permeation" in the 1890s (when the Fabian insurgent John Burgess launched the original Hobsbawmite critique), with the brouhaha of 1906–11 triggered by H. G. Wells's Fabian foray, the trauma of Ramsay MacDonald's "betrayal" in 1931, and the split consequent on the formation of the Social Democratic party (SDP) in the early 1980s. The perennial necessity of the society to redefine itself, its relations with the Labour party and other organizations, the question of membership eligibility, and so on are elucidated here with great clarity as one important strand of continuity since 1884.

A second instance of continuity in the quest of definition, the discussion over the nature and validity of socialism, is only lightly touched on in reference to Richard Crossman and Anthony Crosland, whom she connects with Graham Wallas. That insight is implicit in her several references to Wallas over the course of the book, but it is never pursued. Inexplicably, she did not consult the Wallas papers, although they are readily available in the library where she examined those of Sidney and Beatrice Webb, Edward R. Pease, and F. W. Galton. Wallas's resignation in 1904 and his doubts that socialism would prove to be a "sufficing world-view" point to the "dichotomy" that Pugh describes as opening up in the running debate between Crosland and Crossman and culminating with the resignation or expulsion of the members of the SDP (p. 248). But that ideological fault runs back through G. D. H. Cole's critique of "bastard socialism" to the first Fabian generation.

Some of Pugh's best work is her presentation of the Fabian Colonial Bureau, which benefits notably from her work in organizing its voluminous records. Her book is most valuable where it goes beyond 1959, the period not covered by her predecessors. Chapter 22 is a path-breaking study, the first reconnaissance into the last two decades, although they demand a more astringently critical evaluation than we are afforded here. This is particularly true of the dustup of 1980–83, of which Pugh's account is too brief and too anonymous to be of much use. Perhaps such limitations stem from constraints on a work that was formally commissioned by the society to commemorate its centennial. In any event, what we have is a torso rather than a finished work, valuable to the next scholar in the field chiefly as a means to organize his research.

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DEBORAH EPSTEIN NORD. *The Apprenticeship of Beatrice Webb*. Amherst: University of Massachusetts Press. 1985. Pp. ix, 294. $25.00.

This work reveals the advantages of crossing the boundaries of academic disciplines. Deborah Epstein Nord undertook an analytical study of Beatrice Webb's *My Apprenticeship* (1926) to show its relationship to earlier Victorian autobiographies and its general place in women's writings of that period. She has ended not only by achieving that task superbly but also by producing the most insightful historical study of the leading woman Fabian yet written.

Nord's command of the existing literature is extensive, and she makes original contributions in a number of separate fields. First, she stresses the novelty of *My Apprenticeship* by placing it in the context of earlier Victorian and Edwardian autobiography. Her bold and striking argument is that autobiographies of many intellectual men were set in a pattern: loss of faith was followed by despair, was succeeded by a search for a new faith, and ended in spiritual rebirth. By contrast, women's autobiographies avoided making individual spiritual crises the center of interest, which reflected the encouragement of selfless service and the taboos against self-advertisement, vanity, fantasy, and ambition. Only a few women Victorian novelists (notably, Charlotte Brontë and George Eliot) engaged the issue of spiritual crises and fulfillment, and even then the crisis was usually resolved in marriage, death, or merely a vision of a true synthesis. Nord sees the novelty of *My Apprenticeship* as consisting in Webb's adoption of the masculine tradition of Victorian autobiography as a suitable vehicle for expressing a woman's search for a faith and a vocation.

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Nord carefully points out that, although this did not wholly free the Fabian of feminine inhibitions, it provided "structure-giving themes" to the story of her early life.

Incidentally, Nord's hypothesis—that autobiographies are usually the embodiments of "myths" derived from fantasies and the distortions of time, memory, and prejudice—enables her to expose a number of "myths" professing to explain the achievements of distinguished women in general and to demolish some "myths" of historians, journalists, and novelists about Webb in particular.

The analysis of Webb's early family relationships and the intellectual influences on her pays closer attention to psychological connections than has any earlier study. Nord reveals how Webb's ambivalent attitude toward her family engendered personal conflicts and how her diary, begun when she was eleven years old, provided a means of self-expression, which left her with an acute awareness of the conflict between the "ego that denies" and the "ego that affirms." A brief but intense love affair with Joseph Chamberlain ended in her abandoning the feminine ideal of fulfillment in marriage and children. Her initially passionless marriage to Sidney Webb was a "working relationship," which Nord compares illuminatingly with resolutions in the novels of Brontë and Eliot.

The evolution of Beatrice Webb's social ideas from her association with the Charity Organisation Society in the early 1880s to her acceptance of Fabianism at the beginning of the 1890s is handled with skill and judgment. Nord compares Webb's own account in *My Apprenticeship* of her debts to and her criticisms of Comte and Spencer, Charles Booth, the Cooperators of the North, and the Fabians to the account in the diaries and subjects the material to shrewd critical comment. Nord concludes that Webb's "joining with the Fabians was no accident, no mere result of her engagement to Sidney Webb; it was a step that followed logically and organically from the 'stage' of socialist thinking she had reached in 1889" (pp. 202–03).

Webb's decision to turn her diary notes into an autobiography was made at the beginning of 1917. Nord argues that this plan arose paradoxically out of Webb's growing disillusionment with Fabianism and out of certain tensions in her marriage. Its "myth" was the reassertion of optimism in a time of increasing pessimism. The origins of her pessimism are diagnosed as arising from her neurasthenic depression induced by the horrors of war and from her doubts about the spiritual force of British socialism. At the personal level, Nord claims, Webb had come to feel that her marriage, in spite of its close comradeship and intellectual productivity, failed to provide full sexual satisfaction and sufficient freedom for her individual expression. Sidney's atheism and his unwavering optimism were antipathetic both to her lifelong search for a religious faith and to her growing disenchantment with politics. But could not the idea of publishing *My Apprenticeship* have represented a genuine reaffirmation of faith in Fabianism rather than a "whistling in the dark"? By early 1917, Webb was recovering from her war depression and becoming more active in Fabian Society and Labour party affairs than she ever had been before. The real period of disillusionment for the Webbs came somewhat later, in the 1920s and 1930s. In her final chapter Nord discusses the Webbs' pilgrimage to Moscow. With discrimination and balance, she explains how and why the Webbs deliberately set out to find Fabian virtues in this "socialism in one country," although they were not blind to the dark side of the Soviet state. Their quest is neatly connected with the earlier analysis of Beatrice Webb's ideas and rounds out a spirited and well-argued book.

A. M. MCBRIAR
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ROLAND HUNTFORD. *Shackleton*. New York: Atheneum. 1985. Pp. xx, 774. $29.95.

Ernest Shackleton's footslogging struggle to within ninety-seven miles of the South Pole and the even more devastating trials that followed his failed attempt to cross the Antarctic continent are among the great sagas of human fortitude and intrepidity. They are told here with consummate skill by a journalist turned scholar who knows of ice and oceans. He makes the reader feel the exhilaration as well as the suffering of men stretched to unimaginable limits of endurance.

Unlike his rival Robert Scott, who was the subject of an earlier biography by Roland Huntford (*Scott and Amundsen* [1980]), Shackleton was a natural leader of enormous magnetism and tenacity. Yet both men shared an almost criminal disregard for expertise and advanced technology. For their blithe expectation that true British grit would see them through every adversity, Scott paid with his life and Shackleton with repeated failure. The author sees this as a direct reflection of national character, specifically of Edwardian England but by inference contemporary Britain as well. In the end Captain Scott, again portrayed here as a jealous and dishonest hypocrite, achieved the greater stature in the British pantheon of heroes simply because he paid the ultimate price for his incompetence, whereas Shackleton survived, as did all the men who ever served under his direct command.

Huntford has given us the most exhaustive and thoroughly researched biography of Shackleton yet published. It benefits from newly available diaries of

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the explorer's companions on the ice. Such sources are all the more important because Shackleton was an irregular and laconic diarist, and the two books he wrote describing his adventures were prepared with the help of a ghostwriter. In addition, by the terms of their contract Shackleton's men were obliged to show him their diaries at the conclusion of the expedition. Thus, the many admiring observations of their leader, though probably both genuine and deserved, are necessarily called into question.

Inexplicably in so comprehensive a biography, there is virtually no mention of science. The reason may be that Shackleton was uninterested in, even contemptuous of, any diversion from his immediate objective. Nevertheless, with scientists of the caliber of Raymond Priestly, Edgeworth David, Douglas Mawson, James Wordie, and James Murray as expedition members, some attention to their considerable accomplishments would seem to be a proper part of the story. Oddly, too, given the voluminous source material on which the author has drawn, several crucial matters are without citation—for example, the assertion that Scott "ordered Shackleton to change his [1907] plans, leaving the entire Ross Sea region to him" (p. 161). Shackleton's efforts to comply at least in part with this outrageous demand almost certainly cost him the triumph of being first to reach the South Pole.

Shackleton's only happiness seemed to be in leading men through excruciating travails. At home in England, he was invariably restless and frustrated, unfaithful to his wife, and full of schemes for quick riches and the realization of his immoderate ambitions. By Huntford's account, he was mentally lazy, rich in the capacity for self-delusion, and hasty in his judgment of men (although he was blessed with two lieutenants as remarkable in crisis as himself—the indestructible Frank Wild and Frank Worsley, navigator of the astounding open-boat journey to Elephant Island and thence to South Georgia). Shackleton's Irish charm was legendary, and he was widely admired. Yet his expeditions were invariably underfunded, and he was never out of debt. Despite his great physical endurance, he probably suffered from heart disease throughout much of his life and died at the age of forty-seven. Future biographers may differ from Huntford's interpretation of this life, but they are unlikely to enlarge substantially on his facts.

PHILIP W. QUIGG
Northport, New York

B. S. MCBETH. *British Oil Policy, 1919–1939*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. xvii, 171. $29.50.

This is an inadequate book about an important subject. Until recently British oil policy in the twentieth century has received scanty attention compared to the domestic and foreign oil policies of the United States. Fortunately, over the last decade this situation has changed. My book, *The State and the Emergence of the British Oil Industry* (1981), examined British government oil policies between 1900 and the late 1920s, using government archives and those of the Shell Oil Company. The first volume of R. W. Ferrier's *History of the British Petroleum Company* (1982) and T. A. B. Corley's *History of the Burmah Oil Company, 1886–1924* (1983) provided first-rate histories of the two significant, wholly British-owned oil companies; in the process they shed much light on governmental oil policies. B. S. McBeth, however, ignores all this recent research. His bibliography lists no work published after 1973, except two of his own studies on Venezuelan oil published in 1982 and 1983. The result is a book that largely repeats known facts and ignores all the new evidence that has emerged from the archives of the oil companies.

The main feature of Britain's oil policy after 1918 was a desire to lessen Britain's dependence on foreign-owned companies supplying oil from nonempire countries. McBeth repeats the saga—already examined in great detail elsewhere—of the government's forlorn attempts between 1916 and 1924 to merge a number of smaller British-owned companies with the large Shell group (40 percent British-owned and 60 percent Dutch-owned) to form a giant, all-British oil company. He also covers briefly the familiar story of British oil companies in Iran and Iraq. The only original aspect of McBeth's book is its emphasis on the importance of Venezuela as a source of Britain's oil supplies in the interwar years, although this importance was not the result of any conscious policy of the British government.

McBeth seldom ventures beyond narrative to analysis, and our understanding of the central puzzles of interwar British oil policy is not advanced. The British government purchased a majority shareholding in the Anglo-Persian Oil Company in 1914, but why did the government subsequently hold this company at arms length rather than use it as a tool of a dynamic oil policy? McBeth provides detailed accounts of interdepartmental disputes on oil policy, but why was British oil policy so incoherent? Why did the British government allow the oil majors to set up more cartels in the British market than in any other in the developed world by the 1930s? Many pages are devoted to Venezuela but only a single paragraph to the Arabian Gulf (p. 132). How did American oil companies in the 1930s secure concessions in Kuwait and Bahrain, sheikdoms in a region of British hegemony? McBeth, who has only consulted the Foreign Office files on Venezuela, provides no answer. And why was the

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government obsessed for so long with the silly notion that the security of Britain's oil supplies depended on British-owned companies and sources of supply? Both the world wars demonstrated that what really mattered was the security of the sea-lanes across the Atlantic.

We need a synthesis on interwar British oil policy that draws together recent research in the field and makes a serious analysis of some central policy issues. McBeth does not satisfy either need in this poorly researched book. It is to be hoped that in the future Frank Cass will do more homework before agreeing to publish manuscripts of this ilk.

GEOFFREY JONES
London School of Economics

RANDLE MANWARING. *From Controversy to Co-Existence: Evangelicals in the Church of England, 1914–1980.* New York: Cambridge University Press. 1985. Pp. xi, 227.

English Evangelicals often appeared as a contentious lot. They questioned other Protestants who recognized the importance of reason, disagreed with Roman Catholicism's appreciation of tradition, and fought with Anglo-Catholics who valued the episcopacy. The glories of the Reformation and *sola Scriptura* become weapons against those who sought to minimize the heritage of pure Protestantism. Randle Manwaring's book tells the story of Evangelicals in the Church of England from 1914 to 1980.

The twentieth century became alien territory for these beleaguered Christians. The Great War created a new world, and the Evangelicals refused to accept aspects of the new environment. A siege mentality, therefore, developed. Biblical criticism, German theology, Anglo-Catholicism, humanism, and ecumenism became the enemies, and the Evangelicals retreated within the comprehensiveness of the Anglican church. Isolated in their villages, they were comforted by the words of the sermon. They eshewed intellectual life and refused positions of leadership and responsibility in the Anglican church. Consequently, the Evangelicals constituted a church within a church. "They *belonged*, as little as possible, in ordinary society and their sub-culture was of their own making" (p. 54).

The debates surrounding a revised prayer book touched a sensitive Evangelical nerve, but the parliamentary defeats in 1927 and 1928 gave them the courage and confidence needed to shed the cocoon of defensiveness and social isolation. But the anxieties associated with the interwar period, "the sour milk of liberalism" (p. 64), and John A. T. Robinson's *Honest to God* (1963) threatened to bring the Evangelicals to the battlements. The National Evangelical Anglican Congress at Keele University in 1967, however, marked a watershed. Evangelicals realized the importance of their contributions and expressed their commitment to Anglicanism, which began to take them seriously. Coexistence had replaced defensiveness, and Manwaring proudly describes this new spirit: "the quarrelsomeness of earlier days seems to have vanished and a somewhat uneasy living together has taken its place" (p. 211).

Manwaring has written an apology for English Evangelicals. He maintains that the Evangelicals saved Anglicanism from corruption, and he also attacks anything that deviated from their creed. His arguments are repetitive and his language biased. The author did not develop the personality of any contemporary leader such as Bishops Handley Moule, Edmund Knox, and Christopher Chavasse. His treatment of the Reverend John Stott is adequate, but the influence of American Billy Graham receives more attention.

The author failed to use personal papers, sermons, the religious press, and pamphlets. Histories of Anglicanism are limited to the works of Stephen Neill, Roger Lloyd, and Horton Davies. Parish life and rural activities receive little attention. Discussions on education, liturgical debates, and Bishop Robinson, however, are outstanding and capture the tensions and problems that plagued the Evangelicals. The Evangelicals contributed much to Anglicanism, but Manwaring's account has shortcomings. His rhetoric is more appropriate for a pulpit.

RENE KOLLAR
Saint Vincent Archabbey

HEIKKI SAARI. *Re-enactment: A Study in R. G. Collingwood's Philosophy of History.* (Acta Academiae Aboensis, series A, Humaniora, number 63, part 2.) Åbo, Finland: Åbo Akademi. 1984. Pp. 141. 65 mk.

R. G. Collingwood always felt painfully neglected by the British intellectual establishment of his time. It is therefore curious that, although, since his death in 1943, he has been widely acclaimed as the most important and perhaps the only major British philosopher of history of the twentieth century, it is not his fellow countrymen but foreigners—Americans, Canadians, Danes, Dutchmen, Norwegians, Australians—who have done him the honor of offering serious commentary on his work. Now from Finland comes yet another critical monograph, the main aim of which is a further clarification of Collingwood's conception of historical understanding as a reenactment of past experience. Heikki Saari has little to say that is startlingly new, and he admits as much. He does, however, review perspicuously the main lines of recent interpretation of the thread that binds together all of Collingwood's later works on history.

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A central issue in the recent controversy about the reenactment doctrine is whether it is intended to make a methodological or a conceptual point. Saari sides firmly with those who opt for the latter alternative, which aims to articulate what historical understanding consists in, not how it is to be achieved. The most simple-minded of the methodological interpretations, as Saari indicates, is the contention that reenactment is an exercise of historical intuition, a contention that he easily shows meshes badly with many Collingwoodian texts. But even less naive ones, he points out, never rise to the challenge of stating anything like rules for achieving understanding via reenactment: this silence is surely significant. Attempts to combine a methodological and conceptual interpretation are represented as equally dubious.

A number of associated Collingwoodian doctrines are discussed, though briefly. Signs of relativism or historicism in Collingwood's writings are put aside as not directly relevant to the task in hand, although how this aspect of his thought about history can be reconciled with the reenactment doctrine is not made very clear. For that doctrine seems objectivist, at least in the sense of assuming that we can recover the past, if not as it really was, then at any rate as it really appeared to those who lived through it; the notion of being relative to the standpoint of the historian seems to have no role to play. Collingwood's attempt to assimilate causal explanation to reenactment is ruled unacceptable by Saari, though not for any clearly sufficient reason. The problem, as he sees it, seems to be whether that is all that causal explanation in history can be. The conventional view that Collingwood was committed by his reenactment doctrine to methodological individualism is denied by Saari and, I think, with plausibility. Readers of this journal may well wish, however, that this issue had received more extensive treatment. Saari does offer useful elucidation of the way reenactment must be "critical" and why it was correct for Collingwood to hold that only "reflective" thought is reenactable: it turns out that for Collingwood all thought is reflective.

A distinctive feature of the book is its frequent citation from Collingwood's unpublished manuscripts, which were only made available to scholars in the Bodleian Library in 1978. No crucial reinterpretations seem to hang on such citation, but these references are often of value in confirming and deepening our grasp of what Collingwood's published writings sometimes say more cryptically. On the whole, this is a good book on Collingwood's philosophy of history. It speaks more directly to the interests of philosophers, however, than to those of the general reader in theory of historiography.

WILLIAM H. DRAY
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ALEC CAIRNCROSS. *Years of Recovery: British Economic Policy, 1945–51*. New York: Methuen. 1985. Pp. xiv, 527.

Since the British thirty-year rule has opened up the official records of the immediate postwar years to scholars, the work of revision and reinterpretation has gone on apace. The present magisterial volume covering the economic policies of the Labour administration is written by Alec Cairncross, who had inside access to many of the crucial debates, although he modestly keeps his own role in the background and relies largely on published memoirs and the official papers now available. His balanced, temperate account takes the edge off many of the controversies of the day: it appears that, after an interval of more than three decades, the successes were less glowing, the failures less disastrous, and the exciting controversies less consequential than their participants imagined.

This work is not a history in the strict sense, nor does it attempt to cover all the important events and developments of the period. It is rather an essay on the way in which policy and economic decisions were arrived at, given the constraints, the pressures, the knowledge, the misinformation, and the ignorance under which ministers and their advisers had to work. In the light of the specific difficulties affecting Britain's postwar economy, Cairncross's overall judgment on their doings appears to be a moderately favorable one.

The author makes no strikingly new interpretations. On the question of the foreign balance of payments, the most persistently troubling aspect of economic management at the time, Cairncross joins those who assert that the British economy was in general balance much earlier than contemporaries thought, but he also believes that it continued to suffer a dollar shortage for much longer, which was a financial rather than strictly an economic problem. The convertibility crisis of 1947 was thus only in part a problem of convertibility; in large measure it was a problem of speculation in anticipation of a speedy reversal of policy. The author has many interesting things to say on planning and nationalization, although they are difficult to summarize: on the whole, however, he considers that nationalization of most key industries represented less of a revolution than protagonists on both sides made out but that controls were kept on too long, judging by the experience of other countries. His only truly sharp criticism is reserved for the handling of the fuel crisis of February 1947 by Emmanuel Shinwell, the minister for fuel and power. The burden of the sterling area (that is, the attempt to keep the pound sterling as international currency) and of behaving like a great power long after the economic basis to do so was gone (particularly in relation to the

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Korean War) are duly noted. But the devastating effect of switching engineering capacity to armaments while most other countries were busy building up modern industries is not fully brought out, nor are the long-term consequences of cutting back on investment at every sign of difficulty discussed. Thus, the two Labour governments receive good marks for having solved the foreign balance problem in the short term; that they set Britain on a long-term, low-investment, low-growth path receives less than full recognition.

Altogether, this is a useful, lucidly written monograph, which confirms rather than challenges received opinion.

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PATRICK WRIGHT. *On Living in an Old Country: The National Past in Contemporary Britain*. Illustrated by ANDRZEJ KRAUZE. London: Verso; distributed by Schocken, New York. 1985. Pp. 262. Cloth $25.00, paper $8.95.

When I first sat down to write this review, I received the latest packet of offerings from the History Book Club, which invited me to join an exploration of the "great homes and families in England." The relevance of Patrick Wright's arguments was brought home several thousand miles away from its origins and subject, for his book is a collection of "occasional essays . . . seeking to clarify some of the ways in which the past has been secured as a cultural presence in modern Britain" (p. 3).

Although the book does regularly refer to the Thatcher government's mobilization of the past in the present, it is not a study of Thatcher's use and abuse of history but rather a critical examination of the formation of "everyday historical consciousness." The book is written from within the field of social analysis and criticism mapped out by the Centre for Contemporary Cultural Studies at the University of Birmingham. More specifically, the theoretical approach is adopted from the writings of Agnes Heller, an exiled Hungarian philosopher and a former student of Georg Lukács. In Wright's view the nostalgia so apparently characteristic of Britain today is not a simple product of ruling-class ideological domination but rather the result of the transformations and dislocations of the forces of modernity, which for at least a generation have entailed a process of political and economic decline in Britain. The nostalgia of everyday life provides, then, the groundwork for the political use and abuse of the past that has been so evident during the past several years.

The first essay takes up the national heritage as a cultural formation, focusing on the politics of preserving or losing the "great" and "historic" country houses, which the declining aristocracy can no longer seem to maintain. In the same piece Wright considers Shell Oil's several decades of advertising and public relations efforts to link itself to the preservation of the countryside, in the course of which it has hegemonically shaped Britain's historical geography and collective imagination. Other essays look at the fascist and anti-Semitic writings of the novelist Mary Butts (1890–1937), examples of the recovery and preservation of sunken ships and old houses (and their significance), and a process of gentrification in north London (the piece makes excellent use of George Orwell's *1984*). These essays are uneven in quality, but the good ones are very good.

This book is essential reading for those of us in British studies (although it is really about England more than about Britain), but it is worthwhile reading for anyone concerned with the contemporary crisis of history. It shows that the real crisis is not the absence of historical thinking but the form and content of it.

HARVEY J. KAYE
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Green Bay

RAYMOND GILLESPIE. *Colonial Ulster: The Settlement of East Ulster, 1600–1641*. Cork: Cork University Press, for the Irish Committee of Historical Sciences. 1985. Pp. xiv, 270. IR£21.00.

This is the first volume to be published in Studies in Irish History, a new series being sponsored by the Irish Committee of Historical Studies. The volumes in the two preceding series established an enviable reputation for scholarship. If we may judge by the first contribution to the new series, this reputation will be maintained.

Raymond Gillespie's *Colonial Ulster* is a reconstruction study of east Ulster from 1600 to 1641. The topic is examined under a series of headings: the physical and human background, the demographic and economic structure of the region, the tension between local and central interests, and the rural and urban societies that grew up during the period. Finally, in a particularly interesting chapter, Gillespie compares the situation in Antrim and Down with that in other regions of Ireland and, indeed, with early colonial America.

Such local studies of seventeenth-century Ireland present a considerable challenge to the historian because of the scarcity of sources. In this case, numerous new sources have been discovered and well-known ones have been reexamined and imagi-

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natively exploited. At times, the effort to extract the most out of the available material seems a bit overenthusiastic. The orders for a commission for musters in 1560 are applied to the muster of 1630, and the proportion of tenants in particular social strata is then deduced from the number carrying a particular type of weapon. The book includes a table indicating the age and marital status of heirs on the father's death in which the total sample for Antrim is twenty and for Down sixteen. Yet these minor criticisms aside, our knowledge of these two counties during this period has been substantially advanced.

At one level Gillespie provides much new information about local government in east Ulster and about estate management among both Irish and settler families. The Irish are shown to have had difficulty in adjusting to the new economic system imposed on them. The settlers, despite formidable obstacles, managed to organize themselves into tightly knit communities in which landlords and tenants became mutually dependent. Thus, by 1640, the settlers' estates had become profitable. The book, however, has a broader thesis: that the very creation of such local power blocks led to tension after 1610 between the centralizing Dublin administration and the local magnates. This idea helps explain the conflict of the 1640s, yet it surely requires some modification. Often the major landlords of the hinterland and the agents of centralization were the same people. Sir Arthur Chichester, for instance, was a centralizer, although he carved out for himself large estates in both Down and Donegal. Even Thomas Wentworth, the arch-centralizer, clearly aimed at creating a power center for himself in Connacht. This theme, nevertheless, deserves more discussion, and what is important is that Gillespie's work provides a major contribution to local history while taking into account the wider context of Irish history as a whole.

MICHAEL PERCEVAL-MAXWELL
McGill University

JOHN GRAY. *City in Revolt: James Larkin and the Belfast Dock Strike of 1907*. Dover, N.H.: Blackstaff. 1985. Pp. xii, 255. $9.95.

When war broke out in 1914, European socialists were, with one or two honorable exceptions, soon revealed to be every bit as nationalistic as the workers whose jingoism they had claimed to despise. John Burns, the British politician whose own socialist fervor had long since been quenched, was moved to comment in his diary—and not without feeling since he, at least, remained true to his pacifist convictions—that "nationalist blood is thicker than socialist water." That was a conclusion to which the Belfast dock strike in 1907, the subject of John Gray's book, had also pointed. It has long been acknowledged and greatly regretted by generations of liberal-minded scholars and activists that James Larkin's success in bridging the sectarian divisions within Ireland's industrial heartland collapsed so swiftly. Little of permanence remained, save the hope that what was possible in 1907 might still provide a way out of Ulster's apparently endless internecine strife. How else can one explain the appearance of yet another book on this most well-documented period of Irish labor history, for well documented it is, Gray's claims to the contrary notwithstanding (p. x). Yet, if the author cannot really hope to be absolutely original, he does present the most thorough and well-informed survey of the 1907 strikes to date.

The narrative sustains the reader's interest throughout, especially in its coverage of the police mutiny to which the strike led. The introductory chapters present a vivid account of Edwardian Belfast's working class. Gray skillfully blends material and analysis from a comprehensive range of sources, although he apparently overlooked John McHugh's article "The Belfast Labour Dispute and Riots of 1907" (*International Review of Social History* [1977]: 1–20). Larkin, of course, occupies center stage during the narration of the strikes of carters and dockers, although occasionally he is invested with a farsightedness that seems to verge on dramatic license. Was he so consciously a revolutionary in search of a revolution (pp. 3, 210)? In Gray's presentation, James Sexton, leader of the dock laborers whose stance ultimately did much to undermine Larkin's efforts, remains as demon king, but he is closely supported by two other villains. Joe Devlin and Thomas Sloan, respectively Nationalist and Independent Orange MPs for west and south Belfast, had unsavory roles, exploiting the already collapsing strike for sectarian advantage.

In its early days the British socialist movement had about it an aura of mysticism, a poetic, quasi-religious quality. Larkin's misfortune was, perhaps, that by the time he brought this vision of socialism to Ireland that mystique had been largely commandeered in the interests of contending nationalisms. This is the main lesson to be drawn from Gray's readable and sensitive book.

KENNETH D. BROWN
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CHRISTIAN JOUHAUD. *Mazarinades: La Fronde des mots*. (Collection Historique.) Paris: Aubier. 1985. Pp. 287. 98 fr.

When I agreed to review this book, I assumed that it was an analysis of the five thousand *mazarinades*

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written between 1648 and 1653. I was mistaken. This book is both more and much less than such a study. It is more in that a new approach is claimed for the study of this material; it is less in that only a small proportion of the pamphlets are considered. Christian Jouhaud and Denis Richet (who provides a foreword) concur in the belief that it would be useless to subject the *mazarinades* to any sort of a quantitative analysis because, as Jouhaud insists, "they do not reflect public opinion and can only very poorly serve the writing of a history of political ideas" (p. 38).

To demonstrate this Jouhaud considers the case of a Parisian pinmaker who in August 1652 referred to a company of militia returning from guard duty as a group of Jews going to their synagogue. Insulted, the company obtained an order for the pinmaker's arrest. In carrying out this directive, the pinmaker was either accidentally killed or tortured and murdered. The episode led to twelve *mazarinades* that were designed to discredit the pro-Fronde militia company. Since the company was composed largely of secondhand dealers who were thought to have profited from the troubles of the time, the repeated allegations that they were Jews carried weight even though Parisians knew that there were no Jews or synagogues in Paris. Jouhaud believes that these obviously false charges "prove to us that the aim [of the *mazarinades*] is not to convince the reader, but at the very most to captivate him, to make him participate in his turn in spreading the rumor" (p. 52).

Jouhaud then considers a placard telling how two sisters near Bordeaux debated the respective merits of the nobles of the robe and of the sword. When the younger sister, who was married to a councillor in Parlement, offered irrefutable arguments in favor of the robe, the elder, who was married to a count, attacked her but was killed in the fracas. The count then challenged the councillor and was himself killed. The robe in Bordeaux was associated with the Fronde and the sword with the unpopular, pro-Mazarin duke of Epernon. In every respect the author of the placard treats the robe as the equal of the sword, except that its adherents were portrayed as less arrogant. The only basis of fact in the story is that there was a duel between a marquis and a councillor in which no one was killed. The purpose of this and other *mazarinades* was not "so much to convince, as to persuade to act—by gesture, word, or thought" (p. 93). It was a "Fronde of words, not a Fronde of ideas" (p. 237). The *mazarinades*, Richet tells us, were not "texts of opinion but . . . texts of action" (p. 13). Even in Bordeaux, there were a few radicals among the frondeurs but no revolutionaries. Their movement consisted of a number of factions struggling to control the monarchy, not to overthrow it.

Richet praises Jouhaud for inventing a new method, but is it really so new? Just as others have before him, Jouhaud discusses a handful of pamphlets that fit into his scheme. A different set of pamphlets persuaded Paul Rice Doolin that the frondeurs were constitutionalists, and Sal Alexander Westrich found in a few of them evidence that the Ormée of Bordeaux was revolutionary. Jouhaud's conclusions about the Fronde are couched in new words, but, reduced to a more traditional vocabulary, they are not too different from those of some older historians such as Ernest Lavisse. Jouhaud has given us a Fronde of words, a Fronde of factions, a Fronde of action, but at the same time a Fronde without substance or purpose.

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ROLAND MOUSNIER, editor. *Un nouveau Colbert.* (Actes du Colloque pour le Tricentenaire de la Mort de Colbert.) Paris: C. D. U. and SEDES. 1985. Pp. 338.

The historians of France who are French have, with few exceptions, a much more positivist outlook on their field than do their American colleagues. An occasional fact might be picked out of the works by Adolph Chéruel, Pierre Clément, or Ernest Lavisse, but these historians are never really read or cited as part of the interpretive framework of the field. By contrast, the historians of France in America not only read but almost revere works by the great French historians of the nineteenth century.

This difference in outlook comes immediately to mind when reading the work under review. Colbert is a traditional subject revived because of a three-hundredth anniversary, yet the theme is the "new" Colbert. Clément would not have known some of the facts presented in this book, and some of the authors shift the emphasis of the traditional interpretation and present it in slightly new jargon. But certainly no new Colbert emerges from these studies. The strain to come up with something new threads through many of the papers. Indeed, almost like ghosts, Chéruel and Clément hover over the discussions, although no one dares to admit that the controversies over Colbert are moribund. Only Roland Mousnier and, with his typical modesty, Michel Antoine grope toward an understanding of what Colbert was about in the history of the state. Mousnier's firm, almost scolding voice comes through the printed words as he reminds the other participants in this colloquium that Colbert must be interpreted as a great administrator whose place in history is assured by the intensity, vitality, and perseverence of his attempts to enhance the power of the state. Of course, precedents may be found for this or that reform, but what is impressive about

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Colbert is the intensity and totality of his program, as well as his refusal to be derailed by one or another seemingly insurmountable problem. The most important fact that can be inferred from this volume is that historians working on seventeenth-century France, except for Mousnier and Antoine, no longer perceive what the state was, or perhaps is, in historical terms. Although the historians whose works are collected in this volume do not represent every school or tendency in historiography, it is nevertheless true that administrative history and state history are no longer comprehended by scholars working in the era of state building. To be sure, no annalistes or anthrohistorians participated in the colloquium, but, had they done so, the comprehension of the history of the state would not have been increased.

Among the papers, Klaus Malettke's supports Jean Meyer's emphasis in a recent biography of Colbert on the importance of abstract ideas and opinion formation in state building. Antoine touches an important theme when he describes Colbert's preference for *commis* over *officiers*, a crucial factor in modern state building. Daniel Dessert breaks new ground by describing the Fouquet-Colbert clash in gang war terms, with Colbert having to fight to survive by finding a scapegoat to prosecute because he himself had been Mazarin's personal financial intendant. Gang warfare must certainly be considered analytically a step beyond clientage, fidelities, and family interest. Dessert understands seventeenth-century politics as a matter of life and death, and his views, along with emphasis on the role of abstract ideas, constitute the original features in these papers. More typically, most conferees distanced themselves from Colbert by condemning the brutality and arbitrariness of the Fouquet trial—moralizing that scarcely increases understanding of the event.

Marguerite Boulet-Sautel's summary of Colbert's role as a patron of legal reform lacks the jurisprudential context in which this reform movement occurred. The author appears to be unfamiliar with William F. Church's significant article on the question. Arlette Lebigre's deft summary of the history of the use of *commissions* supports the conclusion that unintentional shifts occurred from "ordinary" to "extraordinary" justice.

Vladimir N. Malov evokes but does not elucidate Colbert's thinking about utility when he raises the question about whether monastic communities should have been allowed to increase in number. Other essays report some interesting new facts about the decline in tonnage of codfish caught off the Grand Banks, forestation, and naval expenditures to complement the rather familiar summaries of fiscal policy, credit, and domain administration.

The themes of economic protectionism versus free trade and Colbert's attitudes toward war are frequently evoked—apparently because these were the only topics everyone could talk about—but little of interest was said about them. It is evident that several papers came out of rather hasty reading of Clément's edition of Colbert's papers, and apparently the works of Lionel Rothkrug and Herbert Rowen were known only to Malettke, Malov, and Mousnier but not to the conferees in general.

If a new Colbert was not found, of what value was the conference? The record of these exchanges of views should give young historians the courage to undertake research on what can only be described as a congeries of topics—Colbert. And, as Georges Dethan observed, Clément only scratched the surface in his edition of Colbert's papers. There is much to be done, especially in editing and researching the writing of thousands of minor officials and "unknown" Frenchmen who corresponded with the great minister. Only Mousnier became excited by the prospect of editing hundreds of volumes of Colbert papers, for perhaps he alone of all the conferees knows profoundly how the history of the state is the history of everyman.

OREST RANUM
Johns Hopkins University

GUY CHAUSSINAND-NOGARET. *The French Nobility in the Eighteenth Century: From Feudalism to Enlightenment.* Translated by WILLIAM DOYLE. New York: Cambridge University Press. 1985. Pp. 197. Cloth $37.50, paper $10.95.

First published in Paris in 1976 and now translated into English with a short afterword, this essay on the French nobility of the late eighteenth century stresses that in 1789 the second estate numbered twenty-five thousand families or between one hundred ten and one hundred twenty thousand individuals. This is a lower figure than the often-cited three hundred fifty thousand. Guy Chaussinand-Nogaret attacks the Hegelian-Marxist interpretation of the nobility as a feudal brake on the development of French capitalism. He insists that the Versailles courtiers, aristocrats, and financiers (with whose daughters the high nobility often intermarried) were at the forefront of the most dynamic sectors of the economy. After 1760 or thereabouts an outlook based on honor was replaced by bourgeois shibboleths of merit and competition. This wealthy metropolitan elite concerns Chaussinand-Nogaret more than do rich provincials (best represented by the *parlementaires*) and even more than the rank and file of the noblesse. The unbridgeable divide of an expensive education separated the petty "proprietary" gentry, keen to provide their sons with a

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military career, from liberal aristocrats in the first rank of the most innovative and modernizing part of French society. Before 1789, however, culture and marriages increasingly united upper bourgeois and nobles.

Objections to this analysis have been raised. Emmanuel Le Roy Ladurie commented that this book made it difficult to understand the counterrevolutionary activity of nobles. The book's afterword takes up that point. Philippe Goujard denied a growing social fusion between noble and bourgeois, urged the importance of feudal dues in the agricultural economy, and claimed that, rather than introducing bourgeois values to the second estate, the *anoblis* became more like *noblesse*. In general, he believed it wrong to generalize from a part to the whole of the nobility in a way that misrepresented its collective attitude toward privilege. To Roberto Moro the picture of a highly dynamic, capitalist nobility seemed an extravagant paradox that led the historian to claim that the Old Regime nobility predeceased the Old Regime.

The book's major flaw is the inadequate demonstration of the extent of aristocratic capitalism. Examples are often drawn from the series T in the French National Archives dealing with revolutionary confiscations of noble property. Those archives rarely permit calculations of what proportion of family wealth and income was drawn from the revenue of capital. Unlike the French original, the English edition boasts an index, which, though incomplete, facilitates a comparison of nobles identified by Guy Richard as involved in commerce and industry before the revolution with those mentioned here. Very few of them or their children were engaged in similar activities after 1800. Tom Beck's articles on nobles on the 1830s electoral lists showed the nobles rarely paid the business tax. Various wills by nobles after the revolution reveal that, until mid-century, on average no more than 5 percent of noble revenue came from capitalist investments in the broadest sense.

And did the nobility intermarry with wealthy commoners? We lack systematic evidence on marriage patterns of nobles from the mid-eighteenth to the mid-nineteenth century. Anecdotes about marriages between aristocrats and financiers (continuing, one might add, down to marriages with American and Jewish heiresses in the Belle Epoque) show the need for a study of endogamy and exogamy in the nobility. At the level of selected noble families one can stress persistent endogamy as easily as exogamy. If liberal aristocrats stopped believing in their separate identity before 1789, why did they still pursue new titles from rulers after 1800? Had the revolution blighted their enlightened outlook so radically? Chaussinand-Nogaret gives twice as much space to economics as to family, which is odd since family linkages were the main obsession of nobles. Despite many novel insights, this book is misleading in its emphasis on divisions inside the second estate. Certainly, court aristocrats and country gentry had little social commerce, but both shared a pride in history and status, which led nobles, high and low alike, to reject any loss of their distinct inegalitarian identity.

DAVID HIGGS
University of Toronto

DURAND ECHEVERRIA. *The Maupeou Revolution: A Study in the History of Libertarianism; France, 1770–1774*. Baton Rouge: Louisiana State University Press. 1985. Pp. xiv, 347. $32.50.

The reign of Louis XV closed, as it had begun, with the parlements—those self-proclaimed custodians of the traditional "liberties" of France. For, as Franklin Ford and James Hardy have long since argued, the parlements, in assisting Philippe d'Orléans to break the will of Louis XIV and assume the regency, effected a counterrevolution against the gains of absolutism. Sixty-five portentous years later, in 1770, Louis XV's loyal servants, the chancellor Maupeou and the Abbé Terray, reversed the tendency by dismissing the parlements, whose judiciary functions they ascribed to less pretentious and more subservient courts and whose consultative—or protoparliamentary—claims they abolished. Louis XVI would, in turn, restore the parlements on his accession. But, as Durand Echeverria argues, the brief interlude of parliamentary "vacation" carried weighty consequences.

Few will challenge Echeverria's insistence on the significance of the long-standing duel between crown and parlements for the development of prerevolutionary French political thought, although many will challenge various aspects of his argument. Echeverria has taken the royal road of the history of ideas. The Maupeou revolution invoked by his title consists of a revolution in thought. Social and political developments receive scant attention. He even passes lightly over the specific viability of the new judicial system. His purpose is to document the emergence of what he idiosyncratically labels the "libertarian" tendency in eighteenth-century thought. I find his use of the term "revolution" congenial but remain unconvinced by the apparent meanings he attaches to it. Above all, his use of "libertarian" requires justification. Libertarianism appropriately designates many aspects of British as well as French cultural and political attitudes in the eighteenth century but remains ambiguous as a historically specific category of political theory.

Echeverria argues that, in the wake of the abolition of the parlements, political thought diverged

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into three currents represented respectively by the monarchists, the parliamentarians (or "patriotes"), and the independents, whom he readily admits constituted at best a very loose group. The dialectical interaction of the ideas of the first two groups resulted in what he calls the "democratic third position" (p. 302). Echeverria's ascription of individuals to groups follows recognized lines: he identifies Voltaire, Linguet, and the physiocrats with the monarchists; Montesquieu with the "patriotes"; and Diderot with the independents. As he allows, the taxonomic problems remain formidable, and any choice must invite qualification. Nonetheless, the choices also inescapably betray attitudes toward the significance of the specific referents of political terms. Echeverria's taxonomy thus obscures the complexity—not to say contradictions—of the thought of the physiocrats on the one hand and of Diderot on the other. And what about Rousseau?

The ideological origins of the French revolution admit no simple models. Echeverria has cogently reminded us of the bankruptcy of unilinear theories about the rise and triumph of liberalism. He has also offered a thought-provoking preliminary attempt to rethink the complexities. And he has wisely linked the crucial developments in political thought to political events. If some remain unsatisfied by his specific discussions, and even by the problems he chooses to highlight, all must welcome his attempt to provide an overview of the political crucible of what Michel Vovelle has so aptly termed the *tournant des mentalités*.

ELIZABETH FOX-GENOVESE
Emory University

DAVID M. GORDON. *Merchants and Capitalists: Industrialization and Provincial Politics in Mid-Nineteenth-Century France*. University: University of Alabama Press. 1985. Pp. ix, 249. $29.75.

Unveiling economic interest behind political action is not quite as fashionable these days as it was some years ago in social history. But David M. Gordon has demonstrated, in this significant contribution to the social and political history of the Second Empire, that economics has much to say about the political process when conceived in a complex, nondeterminist relation to political actors, events, and issues. In a sometimes brilliant comparative analysis of composition, interest, and politics of the elite in two French cities, Reims and St. Etienne, Gordon shows why economic modernization proved incompatible with political repression in the local politics of these two industrial towns under the Second Empire. Modernization favored the emergence of an economically innovative and enterprising manufacturing elite that found the restrictions of imperial rule inconsistent with the elite's budding political ambitions and self-confidence as producers and employers. A variety of local grievances, both economic and noneconomic, against policies and favorites of the imperial regime provided members of the elite a handy assortment of allies, not least among workers, in the constitution of a liberal opposition. Their dynamism and wealth reinvigorated the politics of notables, which had been in eclipse since the end of the Second Republic, and focused middle-class opinion in a liberal direction, which was more compatible with moderate republicanism. Their success in maintaining the focus of discontent on the empire and in defraying serious radical counteropposition largely explains the relatively smooth transition to a republic dominated by moderate bourgeois interests.

The argument, though not entirely new, refocuses attention on notables alienated by the imperial policy of official candidacy. It also emphasizes the critical role of younger Orleanists in the constitution of opposition in these industrial towns. But what is especially fine about this study is its articulation of distinctively local patterns of opposition and especially the logic by which industrialization informed constituencies and strategies of opposition in different ways in the two cities. Gordon identifies a variety of incisive contrasts, which he uses to develop patterns of social and political formation that can be analyzed with appropriate focus on salient features. Thus, his comparsion is exceptionally well constructed and yet remains sensitive to the interplay of the various factors. For example, he shows that the economic isolation of the steel manufacturers of St. Etienne from the ruling elite of ribbon manufacturers explains not only their interest in political opposition but also their willingness to support strikes of workers as an opposition strategy. The explanation also takes account of the structure of labor and traditions of labor militancy, of affronts, both old and new, to small shopkeepers and other lower-middle-class groups, and of the impact of imperial governmental policy and economic development on small manufacturers in other trades. The account of Reims is equally attuned to various factors, including the personal leadership of the town's mayor and the economic integration of industrial elites, which go far to explain the unwillingness of this city's progressives to encourage labor unrest against fellow manufacturers for political ends.

Gordon uses a variety of archival sources, including death inventories, quite intelligently. Some of his assertions about motives, perceptions, and relationships important to his analysis could, however, use more direct empirical support. For instance, he explains liberal industrialists' diminishing fear of labor militancy by conditions in their industries, without clarifying whether this is an observation of

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their own perception or simply his interpretation of their behavior. His assertion of an association between the disunity in Reims's workers' organization of the late 1860s and structural dichotomies of the labor force is not matched by evidence of one reflecting the other. More generally, the question may be raised of the extent to which the economically related foundations of opposition in these two cities were necessary reasons for the emergence of an anti-imperial coalition. An argument might be made that the imperial government defined the targets of opposition by its narrow choice of local favorites for official candidacies and itself provided the main reasons for opposition by its policy failures. Gordon explains very well how and why a segment of the local elites capitalized on an opportunity for opposition, but his explanation does not itself account for that opportunity. That, it may be argued, derived more from politics "narrowly" conceived—policies and leadership, both national and local—than from specific economic divisions and grievances.

GEORGE J. SHERIDAN, JR.
University of Oregon

WILLIAM H. SEWELL, JR. *Structure and Mobility: The Men and Women of Marseille, 1820–1870*. New York: Cambridge University Press or Maison des Sciences de l'Homme, Paris. 1985. Pp. xvi, 377. $44.50.

This is quantitative history with a vengeance. After an interesting introductory chapter on the economic development of Marseilles in the nineteenth century, William H. Sewell launches into the analysis of the occupational structure of that city, the occupational pecking order, the social characteristics of neighborhoods, the demography of urban growth, the nature of migration into the city, the relation between crime and migration, and, finally, the question of social mobility for both men and women. The documentation for this study is provided by the computer analysis of two major sets of quantifiable documents: the manuscript census lists (*listes nominatives*) and the marriage registers, which provide considerable information on the social and geographic origins of marriage partners. Both sets have shortcomings and problems that are discussed at length and that had to be corrected in the design of the study. In the case of Sewell's discussion of criminality, these are supplemented by court registers.

The bulk of the book is devoted to walking the reader through one hundred computer-generated tables, graphs, and distribution maps, for, as we all know, computers are splendid at sorting anything every which way. It reminded me of sitting through a university budget committee meeting of unlimited duration: even though you are bound to hit on occasional nuggets of interest, the overall effect is one of skull-splitting boredom. This is undoubtedly the deadliest historical monograph that I have had the duty to read.

The basic problem, it seems to me, is that there is a vast discrepancy between the investment—on the part of the author, the foundations that financed his team of research assistants, and the reader who sits through untold tedium—and the return on that investment. In the first place, the general outline of what Sewell tells is familiar, though not, to be sure, for Marseilles. Back in 1964, Adeline Daumard, in her brilliant *La bourgeoisie parisienne de 1815 à 1848*, demonstrated with a wealth of documentation—including quantifiable data—the dynamic role of immigrants in the growth of nineteenth-century Paris. Sewell does more of the same, and in excruciating detail, for Marseilles. At times the discrepancy between the top-heavy methods and the banal findings provide unintended comic relief, as in his construction of elaborate status scales to discover the obvious. At other times, the author tilts at a straw man, as when he challenges the authority of Louis Chevalier's *Classes laborieuses et classes dangereuses*, a book already widely regarded as a work of romantic fiction clad in scholarly trappings.

Second, this study reflects and nurtures a neopositivist illusion that the facts speak for themselves. A multitude of facts can indeed be generated by a computer that has been fed data from the census lists and marriage records, yet correcting the flaws inherent in such data is a fairly subjective operation. More important, the facts generated do not mean anything *per se*. It is only when the author hypothesizes about illiteracy not slowing down the social mobility of women or about the superior social mobility of immigrants of peasant stock over artisan immigrants that such "facts" assume any meaning at all. Yet these attempts at explanation are, for the most part, purely speculative, however plausible. The paradoxical result is that this seemingly rigorous positivist method yields the most unverified (and often unverifiable) kinds of explanation.

This review is not intended to cheer on the Luddites against the quantifiers. Sometimes an inherently interesting and clearly defined hypothesis can only be demonstrated or disproved by quantitative methods. I think of Charles Tilly and Louis Tilly's analysis of collective violence in the light of established paradigms, of Lawrence Stone's testing of the openness of the English elite, and, for that matter, of Sewell's own earlier attempt to explain the political polarization of the Marseilles working class in 1848. What quantification does not sustain well is the sort of open-ended, descriptive analysis that Sewell has attempted in this book. I dread to think of teams of other researchers leaping onto the

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same bandwagon, for the supply of census lists and marriage registers is inexhaustible. If they do, we shall have to paraphrase T. S. Eliot: the world will go with neither a bang nor a whimper, but with a yawn.

PETER H. AMANN
University of Michigan, Dearborn

ELIZABETH ANNE MCCAULEY. *A. A. E. Disdéri and the Carte de Visite Portrait Photograph.* (Yale Publications in the History of Art, number 31.) New Haven: Yale University Press. 1985. Pp. xvi, 262. $30.00.

This monograph on the history of French photography is based on extensive archival research, includes over two hundred illustrations, and makes a noteworthy contribution to the study of mid-nineteenth-century culture and society. Elizabeth Anne McCauley centers her analysis on the career of A. A. E. Disdéri (1819–89), who made a fortune during the Second Empire by mass-producing photographs of the rich and famous (as well as the ordinary and unknown) in his Paris studio on boulevard des Italiens. Disdéri died in obscurity, and most of what was previously known about him is found in the autobiography of his rival, Nadar (Gaspard-Félix Tournachon). Critics, moreover, have generally consigned Disdéri to passing mention as a pioneer of "commercial" photography. The man whose employees churned out sixteen hundred negatives (each holding eight small exposures) during a single month in 1861 seemed forever tainted by his short-lived success as an entrepreneur of images. With scholarly interest now shifting toward the social context of photographers' production, McCauley has found a first-class subject. My only reservation is that she does not succeed in suffusing Disdéri's story with life, although that might require the gifts of a Balzac or Zola.

Disdéri struck it rich with a fad he helped create: the vogue for distributing (or allowing to be sold) *carte de visite* photographs of oneself and collecting them from (or of) others. The inexpensive format gave virtually everyone access to intimate images of famous persons. Everyone who has purchased bubble gum to get a baseball card stands in Disdéri's debt. McCauley is not content to remain at the level of kitsch, however. Among the most interesting sections of her book are discussions of the social and political uses of the *carte de visite* photograph as a genre (the Empress Eugénie was one of Disdéri's most devoted clients) and the ways they were visually consumed when placed in albums modeled on medieval breviaries. The *carte de visite* business was also ideal for producing naughty pictures as well as for capturing the gestures of well-known actors and actresses. McCauley has stimulating things to say about both subjects, particularly on the use of poses "in character" as primary material for theater history.

Given McCauley's training as an art historian, it is nineteenth-century posing itself that holds her keenest attention. "Since his process was still too slow to capture fleeting moments and his large camera was fixed on a heavy tripod," she writes, "the photographer was forced to act analogously to the painter and actively pose his subjects" (p. 136). Her chapters on individual, family, and group *carte de visite* portraits enrich our understanding of the compositional technique of such major artists as Degas, Manet, Monet, and Renoir. The puzzle created by Manet's strange portrait of his friend Pertuiset posed as a lion hunter, for example, is solved by comparing it to a photograph of Pertuiset taken by Disdéri (figures 187 and 188). The entire book raises significant questions about how we look at photographic evidence of the past as well as how the men and women of the Second Empire sought to portray themselves.

ROBERT J. BEZUCHA
Amherst College

OSCAR L. ARNAL. *Ambivalent Alliance: The Catholic Church and the Action Française, 1899–1939.* Pittsburgh: University of Pittsburgh Press. 1985. Pp. xii, 258. $24.95.

For Oscar L. Arnal the relationship between the Catholic church and the *Action Française* between 1899 and 1939 was "a barometer of the internal warfare within French Catholicism between the forces of order and the forces of change" (p. 184), warfare in which the church usually supported order. Catholic leaders shared a "fortress mentality" with the *Action Française* and formed shifting alliances of convenience with it because both despised the government and ideals of Third Republic France and "yearned for a past society which was orderly, hierarchical, authoritarian" (p. 183). Conservative churchmen, lay and clerical, could ignore the agnosticism of Charles Maurras and the anti-Semitism and violence that were the stock in trade of the *Action Française*; Maurras and his minions could disregard the church's lack of devotion to royalism. This history has been recounted frequently before, but Arnal claims to present a new interpretation based on his conviction that the papacy and the Catholic church in France were more reactionary, more authoritarian, and more willing to countenance the excesses of the *Action Française* than has been previously acknowledged.

The major failing in Arnal's argument is his establishment of Christian Democracy, the Sillon, and Marc Sangnier as the benchmarks against which

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to measure the commitment of Catholics to democracy, social change, and internationalism. The men and policies judged to fall short are branded "superpatriots" and "ultraconservative." But constitutional monarchy, limited parliamentarianism, chauvinistic nationalism, and paternalistic social reform were entirely consistent with "liberalism" in pre-1914 Europe. Throughout the entire forty-year period considered, France, despite its egalitarian slogans, remained a very conservative republic. Arnal unwisely dismisses "authoritarianism" as the province solely of reactionaries, although Georges Clemenceau was an authoritarian but hardly a reactionary. Few political groups anywhere in Europe, even those of the Center, could easily meet Arnal's standard of "democratic," and thus he finds it absurdly simple to declare, and to denounce, relatively liberal Catholics as nondemocratic.

Although Arnal provides some interesting new detail about the "bizarre relationship" between the Catholic church and integral nationalism, his emphasis is clearly on French churchmen and lay associations. His analysis of the *Action Française* and its leaders is not at all innovative. Arnal also appears to assume that what an individual published as an editorial, or delivered as a speech or sermon, accurately represented inner conviction. Such reasoning leads to fallacious, or at best inconclusive, results. General Edouard de Curières de Castelnau, the leader of the important Fédération Nationale Catholique, is particularly ill-served by this approach and thereby misconstrued. None of the major figures, not even Sangnier, is portrayed in more than a one-dimensional manner. Curiously, the twenty-one interviews that Arnal lists in his extensive bibliography do not inform this monograph in any important manner or provide the complexity of personality and interpretation that are lacking in its aridity.

BENJAMIN F. MARTIN
Louisiana State University

EDWARD MORTIMER. *The Rise of the French Communist Party, 1920–1947*. Boston: Faber and Faber. 1984. Pp. 431. £30.00.

Writing the history of French communism can be a depressing task. Not without its moments of grandeur, the past of the French Communist party (PCF) is full of cryptic twists and turns, cunning lies and willful self-deceptions, strident accusations and counteraccusations, and personalities whose drab mediocrity stands in sharp contrast to the shining ideals they claimed to serve. Perhaps these circumstances explain why Edward Mortimer took over ten years to complete a book that was, as he candidly admits, half finished when he laid it aside in the early 1970s to pursue other projects. The result is an intelligent and straightforward account of the PCF's internal organizational development and its role in the French political system from 1920 to 1947. A brief epilogue carries the story up to 1981, at which point the PCF's fortunes took a distinct, if not unprecedented, turn for the worse.

Mortimer writes well and knows how to confine a serpentine story within the bounds of a smoothly flowing narrative. Moreover, his presentation of the most controversial issues in the party's history strikes me as eminently fair and, for the most part, convincing. For example, I am inclined to think that Mortimer is right when he argues—in contrast to many historians—that, although the PCF's leadership had no plans for seizing power through an insurrection following the liberation, they did consider it possible that the party's organizational strength and strong electoral following could be used to turn France into a people's democracy, a tactic that was successfully implemented in Czechoslovakia between 1945 and 1948.

I could give other examples of Mortimer's judiciousness and sense of just proportion: he writes sensibly and persuasively about the expulsion of Jacques Doriot and the adoption of the Popular Front policy in 1935, the reaction of the party to the signing of the Nazi-Soviet Pact in 1939 and the defeat by Germany in 1940, and the timing and degree of involvement of Communists in the Resistance. Still, I cannot help but express reservations about the kind of history he has elected to write. Mortimer has given us a conventional political account, a story of feudal lords in which the vassals—not to mention the peasants—seldom appear. Furthermore, even on his chosen ground, he disappoints. For, although he devotes hundreds of pages to the internal history of the party leadership, he has almost nothing new to tell and evidently did not bother to track down and interview the many former party leaders who were still alive when he did his research in the early 1970s, a curious omission for the Paris correspondent of the London *Times*.

More serious, in my opinion, is that Mortimer shows little interest in the party's militants and the underlying reasons for their attraction to an imported ideology. Nor does he follow the party's history from Paris into the departmental organizations where its activities escape the generalizations that can be convincingly made about the central apparatus. Yet this is precisely what has to be done in order to shed new light on the rise of the French Communist party. Constrained by these limitations and based almost exclusively on secondary works in French and English published in the 1960s and early 1970s, Mortimer's history thus succeeds only modestly in its stated ambition: to illuminate the

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process by which a party whose origins and vocation were revolutionary became a conservative force. Perhaps Mortimer should have considered the possibility that the contradiction he discerns between the origins of the PCF and its function in French political life is more apparent than real: for between 1920 and 1947 the PCF did radically transform the game of French politics, although in a way that had little to do with the official ideology of the party.

ROBERT WOHL
University of California, Los Angeles

JAVIER REAL CUESTA. *El carlismo vasco, 1876–1900.* (Historia.) Mexico City: Siglo Veintiuno. 1985. Pp. xiii, 338.

The Basque provinces of Spain have been the home of two exceptional political movements in modern times: Carlist traditionalism and semiseparatist Basque nationalism. The Carlist struggles spanned more than half a century to 1876, whereas nationalism only clearly emerged in 1892. Javier Real Cuesta's study of Basque Carlism thus concentrates on a period of transition in which the decisive industrial transformation of Basque society began, at least in the key province of Vizcaya, but in which the distinctive new social and political patterns of the twentieth century had not yet fully emerged. After the final military defeat of Carlism in 1876, the political life of the Basque provinces tended to polarize—though in shifting and rather incoherent patterns—between the elite liberalism dominant in Spain and the still-strong residual loyalty to the traditionalist cause. Carlism retained the loyalty of a large part of Basque rural society and also of the urban and small-town artisan and lower-middle classes, a support that only seriously eroded after the turn of the century.

The book is divided into three parts. Approximately the first half is devoted to the ideological history of Basque Carlism during the period; the second part examines local variations in political performance; and the third part studies local electoral geography and the correlation between socioeconomic morphology and political support. It produces no particular surprises or major breakthroughs, since the general history and structure of Basque Carlism in this era is at least moderately well understood, but it provides the detailed description and analysis heretofore lacking, with careful examination of provincial and district variants.

In general, Real Cuesta's examination reveals highly diversified and shifting patterns in local political strategies and alliances, especially among the Carlists' chief adversaries—the liberals. The constant realignments among Basque liberals and the nascent Left were more intricate than in any other part of Spain because of the heritage of Basque particularism, the Carlist wars, the autonomist tradition of diversity in each of the three provinces, and the changing social and economic structures (developing rapidly in Vizcaya, declining in Alava, and maintaining relative stability only in Guipuzcoa). Although Carlists never gained the majority of Basque seats for the Spanish parliament, part of the time they prevailed in the local and provincial assemblies and provided the most direct element of continuity in the region. The one major split in Carlism during this period—the Integrist schism—is attributed by the author to conflict of personalities and status rivalry rather than ideological difference, although at one point he contradictorily (though correctly) recognizes that the "doctrinal theme" was "the real nucleus of the question" (p. 86). Little effort is made to relate Carlism to Basque nationalism, a theme already treated elsewhere. Real Cuesta accurately emphasizes throughout that Carlism stemmed from the traditional society, whereas nationalism was indirectly the product of the new industrialization only beginning to develop at this time. In general this is a useful work, which further clarifies the growing political complexities of Basque society on the eve of the twentieth century.

STANLEY G. PAYNE
University of Wisconsin

CARMEN DE ZULUETA. *Misioneras, feministas, educadoras: Historia del Instituto Internacional.* Zurbano, Spain: Castalia. 1984. Pp. 294.

For the many Spaniards and American Hispanists who continue to meet there, the handsome building at Miguel Angel, 8, in Madrid represents a tangible link with a liberal democratic past that was repressed, if not extinguished, during forty years of Franquist rule. That the Instituto Internacional was created by American "missionaries, feminists, and educators" has, however, generally been forgotten, especially by Spaniards more familiar with its association with the educational reformers of the Institución Libre de Ensenanza. In this book Carmen de Zulueta proposes "to do justice to the founder of the institute and her successors and to restore to them the place they deserve among the pioneers of feminine education in our country" (p. 9).

The International Institute for Girls in Spain was almost singlehandedly created by Alice Gordon Gulick, who accompanied her husband, a Protestant missionary, to Santander, Spain, in 1871. Appalled at the inferior education available to Spanish women, Gulick dedicated her life to re-creating her alma mater, Mount Holyoke Seminary, in Spain. In

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1881 the couple moved to San Sebastian, where their mission school provided young women with training in Froebelian kindergarten methods, then almost unknown in Spain. In the 1890s several of the students prepared successfully for high school and university examinations, another innovation for Spanish women. The school's modern pedagogy and its policy of religious toleration soon attracted the patronage of the liberal Spanish bourgeoisie.

In 1892 the institute was incorporated independently of the American Board of Commissioners of Foreign Missions, primarily to permit it to own real property, prohibited to religious corporations under Spanish law. By dint of constant fundraising across the United States among supporters of women's education, the Gulicks were able in 1902 to purchase property in Madrid near the Institución Libre de Ensenanza, which shared their preoccupation with pedagogical reform.

After Alice Gulick's death in 1905 and the appointment of Susan Huntington as dean in 1909, the institute's connection with Protestant evangelism was severed. The reputation of the college—now shorn of its mission school—increased; the links with the Spanish reformers multiplied. When the First World War diverted American attention from women's higher education in Spain, the institute overcame financial difficulties by leasing its properties to two new Spanish institutions providing higher education for women, the Residencia de Señoritas and the Instituto-Escuela. Some years after the Spanish Civil War, the corporation resumed direct control of its building on Miguel Angel, providing, through its sponsorship of various Spanish and American cultural activities, a thread of continuity with the past.

De Zulueta, who knows first-hand both the American and Spanish educational milieus, has recovered the history of an important cultural institution. For an American reader, the sections sketching the history of Protestantism and women's higher education in the United States will seem unnecessary. Similarly, Spanish readers may find superfluous the excursuses on Protestantism and pedagogical reform in Spain. A more extended analysis of the education of Spanish women at the turn of the century would, however, have provided more context for Gulick's work.

CAROLYN P. BOYD
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HORST BUSZELLO *et al.*, editors. *Der deutsche Bauernkrieg.* (UTB für Wissenschaft; Uni-Taschenbücher, 1275.) Paderborn: Ferdinand Schöningh. 1984. Pp. 418. DM 29.80.

It is hard to imagine history students being better served than German students are by this volume on the German Peasants' War of 1525, which is a clearly written, intelligently organized, and expertly executed guide to a major topic of German national history. Its nine authors, most of them historians who came to maturity around the time of Peasants' War anniversary in 1975, offer here a summary of the great wave of research that produced more than five hundred titles in 1975–76 alone.

The first of the volume's three parts briefly treats historiography (Horst Buszello), current interpretations (Peter Bierbrauer), and the Holy Roman Empire about 1500–20 (Peter Blickle). The second part reviews the revolution's six regional theaters: the Upper Rhine (Buszello), Upper Swabia and Württemberg (Claudia Ulbrich), Franconia and Thuringia (Rudolf Endres), the Middle Rhine (Wolf-Heino Struck), and the Alpine lands (Blickle). A concluding, analytical part handles the revolution's causes (Endres), relations between town and countryside (Ulf Dirlmeier), the movement's legitimation, phases, and goals (Buszello), and its consequences (Helmut Gabel and Winfried Schulze). There is an extremely valuable annotated bibliography of 148 items compiled by Bierbrauer, a synoptic chronology for the regions, a set of superbly useful maps, and a good index. Great pains have clearly been taken to make this volume useful in the classroom, and its comprehensive vision will make it useful to the specialist as well.

Modern writing on the Peasants' War began fifty years ago with Günther Franz (*Der deutsche Bauernkrieg* [1933]), who saw it as the final torrent of a mighty stream of peasant revolts, which since the late 1200s had aimed at the political enfranchisement of the German peasantry. His views began to undergo revision in the early 1960s, first, as Buszello points out, in the German Democratic Republic and only later and haltingly in the Federal Republic. The formulation by Max Steinmetz in 1961, based on an idea of Friedrich Engels, of the Reformation as a "early bourgeois revolution" and the Peasants' War as its peak, was and is the chief new idea on this topic since Franz. The thesis enjoys formal acceptance, though with numerous qualifications, in the Democratic Republic, whereas West German historians are divided. Some, resisting class analysis and finding no other interpretive schema adequate to the phenomenon, assume the stance, which Bierbrauer rightly calls "paradoxical" (p. 27), that, contrary to the perceptions of contemporary witnesses, there was no common thread in the regional and local movements in 1525. There is, however, another way, the path blazed by Blickle (*The Revolution of 1525*) [1981; German ed. 1975]), who synthesizes Franz's conception of the Peasant's War as the last peak of a movement for peasant political life with

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the Engels-Steinmetz thesis of the class character of the movement as a product of the late medieval agrarian troubles and the consequent crisis of feudalism as a political system. Although, as Bierbrauer rightly points out, Blickle's interpretation has not yet won general acceptance, it nonetheless remains the outstanding achievement of the reassessments associated with the anniversary.

The authors of this fine volume write candidly of what remains to be done. Two problems stand out. First, despite the efforts of Steinmetz and Blickle, the Peasants' War (which Blickle calls "the Revolution of 1525") remains unintegrated into the general repertory of European revolutions and the revolutionary tradition. The second problem is both more serious and more capable of resolution, for it concerns the role of religion in general and the Protestant Reformation in particular in the Peasants' War. Largely under Blickle's influence, the current tendency is to trace the religious idea of the "godly law" to Huldrych Zwingli's theology. Franz thought its origins had to be much earlier, also in theology but in the ideas of Wycliffe and the Hussites. What remains perfectly opaque, however, is the religious world of the Swiss and south German peasantries before the Reformation, quite apart from the influence of learned theologies, orthodox or heterodox. Was there, for example, any connection between the religious communalism of the common man and the communal religious rituals of ordinary parish life? Against Franz we must remain skeptical about the influence of Hussitism in the rural communities of the German south; against Blickle we must doubt that the popular idea of Christian community sprang full blown from the mouth of Zwingli into the heads of the peasants, although in fairness it must be noted that his latest book, *Gemeindereformation* (1985), begins to recognize this. On this ground—the heart and soul of the common man—lies the most promising path for a new breakthrough toward synthesis.

This is precisely the sort of book North American students of European social history need to read. It ought to be translated.

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FELIX ESCHER. *Berlin und sein Umland: Zur Genese der Berliner Stadtlandschaft bis zum Beginn des 20. Jahrhunderts.* (Einzelveröffentlichungen der Historischen Kommission zu Berlin, number 47; Publikationen der Sektion für die Geschichte Berlins, number 1.) Berlin: Colloquium. 1985. Pp. xiv, 402. DM 98.

Felix Escher presents a history of the expansion of Berlin into the immediately surrounding territory. The spatial boundaries of the study are determined by a political definition—the territory included in "Gross-Berlin" in 1919—and its conceptual boundaries are determined by its origins as a thesis. Regional history in West Germany remains dominated by an interest in the medieval and early modern periods and by a virtual allergy to economic theory. Accordingly, Escher, having selected the 1919 boundaries, pursues the history of all human settlements within those boundaries back to a dim archaeological past before the earliest written records in 1375. Three long chapters—over half the book—take the story to roughly 1850, although the city had only tenuous relations with and little impact on the districts beyond its immediate boundaries before the advent of the railway. The author certainly includes some interesting details. From the late Middle Ages until well into the eighteenth century most peasants in the region enjoyed holdings of approximately thirty Magdeburger morgen, or 7.66 hectares, and in Falkenburg (on the extreme northeast of the 1919 boundaries) the number of peasant holdings remained constant throughout those long centuries. Nonetheless, although the outlines of the old estates and forests can be seen in the later administrative divisions of Gross-Berlin, most of the material in these chapters is simply irrelevant to the book's main themes. The fourth chapter, according to its title, covers the conventional "upswing" of 1850 to 1873. Actually the chapter extends to 1890, which makes more sense from the standpoint of the pattern of Berlin's development but which apparently does not fit the conceptual scheme Escher feels obligated to follow.

The fifth chapter is entitled "The Wilhelminian Epoch." In fact, Wilhelm makes only three brief appearances, as the patron of a "patriotic building society" founded by the Christian labor unions in 1904, as the donor of the Grunewald park (formerly a royal hunting preserve), and as the force speeding acceptance of the forestry ministry's 1893–1911 plan for a monumental (and militarily convenient) "Heerstrasse" connecting the center of Berlin with Spandau via Charlottenburg. Not surprisingly, Wilhelm seems not to have concerned himself much with the technicalities of land usage and planning in his capital city. Nevertheless, this is the focus of the book, as revealed in the chapter's subtitle, "Apogee and End of Private City Development." The details here are both interesting and relevant. Forestry ministry officials purchased the assent of the Charlottenburg city council to their plan by narrowing the width of the proposed artery from sixty to fifty meters and offering the city the right to purchase two large tracts of open land, whose value all expected to rise dramatically after the opening of the road and accompanying rail connections. Throughout the period of Berlin's most rapid

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growth, central government ministries, the independent authorities of Berlin and the surrounding cities, large industrial firms, and, not least, private property developers all jostled one another, struggling to make the most out of opportunities to speculate in land values by influencing zoning regulations and the pattern of transportation development. The book ends with the creation of the new administrative unit of Gross-Berlin in 1919, however, and only a brief epilogue outlines the new authority's attempts to grapple with Berlin's problems during the Weimar era and Albert Speer's grandiose unrealized plans for a suitable capital for the Third Reich.

Escher laments the absence of a central authority with power to impose a single plan on Berlin's development before the First World War and also the tendency of public authorities to behave like private speculators in profiting from urban development. That is about as far as Escher goes, however; he makes no attempt to explain development or evaluate alternatives using general theories of any sort. The assertions that capital-gains taxes, an overemphasis on luxury housing, and interest-rate movements played a role in bringing the era of "private development" to an end before the First World War, for instance, remain unsupported even by economic argument, let alone by statistical evidence or quantitative testing. Occasional social historical tidbits—for example, the pressure brought by police authorities in 1902 and again in 1910 for riding paths to benefit the large numbers of military officers living in the western suburbs—suggest that the material exists for something more interesting than the abstract and impersonal account of debates over proposed developments that Escher presents. For those interested in social history, Berlin itself, and city planning generally, the fifth chapter will repay study but is apt to whet more appetites than it satisfies.

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WOLFGANG NEUGEBAUER. *Absolutistischer Staat und Schulwirklichkeit in Brandenburg-Preussen*. Foreword by OTTO BÜSCH. (Veröffentlichungen der Historischen Kommission zu Berlin, number 62.) New York: Walter de Gruyter. 1985. Pp. xviii, 715. DM 148.

This book joins a long line of regional studies of eighteenth-century German school reform but provides a doggedly empirical antidote to the earlier tendency to mistake the ambitions and claims of the "absolute" state for a capacity to effect change at the local level. Wolfgang Neugebauer has meticulously combed a fragmentary but imposing mass of records in West German archives, as well as a large collection of local and regional histories. In addition to providing succinct accounts of the stages of school reform, he sifts through a great deal of data on the everyday life of local schools, including the availability of various schoolbooks, seasonal patterns of attendance, and the sources and levels of teachers' incomes. Fortunately, the analysis extends to the academic levels of town schools and to various forms of private instruction. But the focus is on popular education in the hundreds of schools scattered through the villages and towns of Prussia's core province. In the few towns where reform had made some headway by the end of the century, the impetus had come from local initiatives. Most schools had changed little, if at all; they still bore little resemblance to the recognizably modern institution that state-sponsored reform aimed to impose. A skillful use of other regional studies suggests that Brandenburg was not atypical in this regard.

Although these conclusions may come as no surprise to some readers, their demonstration in detail enhances our understanding of the discrepancy between the legal reach of the eighteenth-century state and its administrative grasp. Nonetheless, the return seems too modest given the magnitude of the investment and the rich potential of the material. This monograph strengthens my impression that the integration of the history of German education into the new "social" history remains tentative and partial. Since the very framing of the inquiry in terms of research on absolutism artificially restricts Neugebauer's vision, the local school, even when it is being set within "the village nexus" (p. 468), figures primarily as an object of state policy. His concern is more with identifying the social and economic obstacles (familiar enough from other studies) to a reformed school than with explaining local arrangements and practices in terms of local culture. For our glimpses of that culture we are often asked to take at face value the observations of clergymen and educators, who judged it by their own standard of rationality and lamented its stubborn ignorance and backwardness.

Conspicuous by their absence are questions that recent historical research on popular culture, particularly from anthropological perspectives, might have been expected to raise. We no longer conceive of popular culture as a discrete jurisdiction "below" literacy. How was the relationship between the literate and the oral affected by the limited dissemination of reading and to a lesser extent writing through the school? Had memorization of the catechism been assimilated into oral traditions, or did it help appropriate those traditions under a state-imposed orthodoxy? Clearly, instruction in the traditional school was ancillary to informal education in the patriarchal household, but how did the school support (or modify) the organization of the peasant

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family as a labor unit and the cultural norms sanctioning that organization?

My point is not that Neugebauer should have written a different book but that such questions are highly pertinent to his subject. He ends by raising the possibility that from the sixteenth century onward—that is, long before "absolutism"—the school contributed to a process of "social disciplining," conceived broadly as a "social-anthropological phenomenon," in response to "local impulse" (pp. 633–34). But, if we are to understand the local impulse behind the survival and proliferation of schooling in the early modern era, we need to shift our focus from the severe constraints on the development of a "modern" institution to the cultural needs that the school actually served. And why should we have to choose between "local impulse" and "absolutist" imposition from above? Granted, under the conditions of the eighteenth century, the "absolute" state could not make the school an "instrument of rule"; that does not preclude the possibility that over the previous two centuries the school, as impotent as it may seem by later standards, had promoted subtle but inexhorable intrusions of the combined forces of ecclesiastical orthodoxy and state priorities into local life.

I emphasize these issues not to detract from Neugebauer's impressive achievement but to sound a note of alarm. Over the past several decades the history of German education has advanced far beyond a celebratory history of institutions—one that sorts out backward and progressive elements within a present-minded teleology and that exaggerates the impact of reform agendas on "school reality." But there is a danger of settling into a new kind of insularity; a concern with social conditions "on the ground" has been incorporated all too comfortably into an institutional approach to power from the top down. In the symbiosis of history and anthropology that has been recasting the study of community and *mentalité*, the field is confronted with new possibilities. Is it not time to ponder them?

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WOLFRAM SIEMANN. *"Deutschlands Ruhe, Sicherheit und Ordnung": Die Anfänge der politischen Polizei 1806–1866.* (Studien und Texte zur Sozialgeschichte der Literatur, number 14.) Tübingen: Max Niemeyer. 1985. Pp. xiv, 533. DM 138.

One of the most powerful legacies of the era of the French revolution and Napoleon in Germany was the growth of popular political movements that sought to influence the policies of the various German governments on a wide range of public issues. Correctly interpreting all such movements as aiming ultimately at the subversion of the monarchical-aristocratic monopoly of the exercise of public authority itself, these governments worked to eliminate or control this danger at its roots by the most basic means: suppressing attempts at political organization wherever they appeared. To accomplish this, they brought into being for the first time a genuine political police, whose origins and development through the first two-thirds of the nineteenth century are the subjects of this detailed examination by Wolfram Siemann.

"Police," of course, not only existed before 1789 in Germany but even helped define the nature of Old Regime absolutism. This was not a political police, however, because it did not include a political task among its functions. That, as Siemann clearly explains, was reserved for a later period when political movements in the modern sense first made their appearance, threatening security and order as defined by autocratic governments. In his analysis of the policies, procedures, and agencies that constituted the political police after 1815, Siemann touches on two levels of development: the arrangements created within the larger individual states of the German Confederation, and those attempted or achieved on a cooperative basis between the states, including the central investigation bodies established at Mainz (1819), at Frankfurt (1833), and in the *Reichsregierung* of 1848–49 and the *Polizeiverein* of 1851–66. The last of these is of special importance. Consisting of the higher police officials of the larger states, it was an extralegal, unofficial, and unpublicized body, specializing in the rapid exchange of intelligence, and it effectively achieved the goal, already noticeable as a tendency in police agencies before 1848, to function preventively rather than reactively against popular political movements.

Siemann finds the organization and functioning of the police in the Habsburg lands more systematic, stable, and consistent than in other states, where the elaboration of the political police apparatus proceeded by fits and starts and was subject to administrative and financial problems, as well as to the personal vagaries of monarchs, ministers, and police officials themselves. Everywhere, however, the pressures of the political task tended to result in a rationalization of police business and a centralization of previously regional police agencies. Connected with this, Siemann argues that a major reason for the quick recovery of public authority by the German governments after the revolutions of 1848–49 lay in the previous experience and "institutional memory" of police officials, especially of the more numerous second rank, and in the special relationship of loyalty and confidence between them and rulers, which helped preserve an ideological

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continuity between the pre- and postrevolutionary period.

Although his book is perhaps overly detailed and (partly for that reason) somewhat unimaginatively organized, Siemann has written a significant study that, in emphasizing the all-German character of the institutional development of the political police before 1866, speaks to some aspects of the background of German unification from a perspective very different from that of the more traditional and "liberal" historiography.

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WOLFRAM SIEMANN. *Die deutsche Revolution von 1848/49*. (Neue Historische Bibliothek, number 266; Edition Suhrkamp, neue Folge, number 266.) Frankfurt a/M.: Suhrkamp. 1985. Pp. 255.

Here is a refreshing approach to a venerable subject: a structural analysis of the good old revolutions of 1848–49. For fifty pages Wolfram Siemann explains how the social, economic, and international systems each suffered a fatal imbalance in the 1840s. In the rest of the book he analyzes the new structures that arose from the resultant revolutionary upheavals and describes how they proceeded to stumble all over one another. He resorts to narrative sparingly, sometimes apologetically, and usually just to illustrate his overall analysis. His general theme is "modernization." He is less interested in the goals and intentions of the individual actors in this historical drama than in their contributions to long-range social processes, processes that appear to have been ineluctable despite the failure of the revolutionaries to achieve their short-term political aims.

There are real advantages to this approach. By assuming the role of quasi-scientific analyst of a vast historical movement, Siemann tries to avoid the usual tendencies to identify heroes and villains, to parcel out blame and guilt, and to mine the historical record to reinforce present-day political attitudes. And, by examining the interrelationships among the elements of this complex event, he makes a pioneering effort to understand the multiplicity of causation that most historians assume to be at work but seldom explore sufficiently. To say that the book does not entirely succeed on either score is not a serious criticism. Siemann does not conceal his strong sympathies for the democratic Left of 1848. But it is impossible for a twentieth-century German historian to view this revolution with authentic scientific detachment, because too much of what was then at issue is still so today. The best that can be hoped for is fair consideration of the political rivals, and on this Siemann cannot be badly faulted. A more important criticism is that he shows no awareness that "modernization," the overall process to which he seeks somehow to relate everything else, is itself a problematic concept that demands discussion, if not definition. But this is, after all, a pioneering effort, and a short book at that. Its many valuable insights give food for thought and demand further consideration.

The most interesting chapter is the one entitled "Revolutionsdynamik," in which Siemann assembles a lot of scattered research of a sociohistorical nature, much of it very recent. What these small-scale studies of associations, petitions, strikes, disturbances, and various forms of *Alltagsgeschichte* have in common is that they illustrate the complexity of the phenomenon we are tempted to oversimplify with the label "revolution." There are reactionary and unclassifiable popular currents alongside the progressive ones and "noncontemporaneous" mentalities that clash with existing structures. These lesser-known aspects are sometimes puzzling and disturbing, but they afford us the opportunity of a more nuanced view of this hoary subject than historical tradition generally provides. We are indebted to Siemann for bringing them to our attention in this way.

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ROBERT LIBERLES. *Religious Conflict in Social Context: The Resurgence of Orthodox Judaism in Frankfurt am Main, 1838–1877*. (Contributions to the Study of Religion, number 13.) Westport, Conn.: Greenwood, for the Leo Baeck Institute, New York. 1985. Pp. xvi, 295. $29.95.

Throughout their long struggle for legal equality in nineteenth-century Germany, the Jews were constantly compelled to prove their worthiness to be admitted to citizenship and to modify characteristics that set them apart as a distinctive group. Christians who spoke out for emancipation expected that the acculturation of the Jews into German society would lead to their conversion. The Jews refused to pay this price and instead debated how far they would reform Jewish religious practices to overcome the alienation of the Christian majority. Impressed by the rapid assimilation of German Jewry, historians have generally assumed that the ideologues of Reform Judaism triumphed and that Orthodox congregations ceased to flourish except in rural villages. Robert Liberles challenges this view in his study of the resurgence of Orthodoxy in Germany after the mid-century. His methodology and revision of earlier interpretations of the religious conflict make his book an important and innovative contribution to modern scholarship.

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Rather than portraying the strife between the Orthodox and the Reformers simply as a battle fought by rabbis in polemical sermons and pamphlets, Liberles places it in the context of social history. This perspective enables him to offer new answers to the question of why Frankfurt am Main became the center for the renewal of Orthodoxy and to correct old accounts, heavily influenced by Orthodox hagiography, which attributed the founding of the movement to the charismatic leadership of Samson Raphael Hirsch, who came to Frankfurt in 1851 to serve as the rabbi of the newly organized Orthodox congregation. In the 1840s the city authorities openly supported the efforts to carry out changes in the main synagogue. The Reformers lost this official patronage in 1848, when the community's lay leadership embraced the aims of the revolution and plunged into democratic politics. After the revolution the Senate shifted its support to the religious conservatives, and the Jewish financial elite lent its wealth and social prestige to the cause of Orthodoxy. The founders of the congregation represented the oldest Jewish families in Frankfurt—a circle of bankers, stockbrokers, and owners of large firms. While Liberles points out that their affiliation with the Orthodox came after a decade of attacks against the Rothschilds and Jewish financial power in the polemics of the political radicals, he shows no less clearly that their intention was to harness modern aesthetics and pedagogy to serve religious tradition and to make their congregation a model for Germany Jewry.

Liberles's probing study of Hirsch's early life uncovers the psychological roots of his uncompromising and militant defense of Orthodoxy. In his childhood in Hamburg he witnessed the struggle between the two parties and the seemingly ineffectual part that his father played in the defense of tradition. He developed an image of himself as a man of courage, prepared to take up alone the battle for God's law. This insight into his personality is relevant to the bitter dispute over separatism in Frankfurt after the enactment of the law on secession from the Jewish community in 1876. Whereas most of the Orthodox in Germany were committed to the solidarity of the community and supported the law as a means of defending minority rights and threatening the Reformers with secession in order to win concessions, Hirsch made separatism into a principle and called on his congregation to resign from the community.

Probably because Liberles seems to be intent on offering a more sympathetic interpretation of Hirsch's behavior than have Noah Rosenbloom and other scholars who have been severe in their criticism, his analysis is not always consistent. He does not explore how Hirsch's pride and perception of his historic role may have led him to act swiftly and without any consideration for the sentiments of his congregation. The concessions that the dissenters to Hirsch won in their negotiations with the community's board were not so much a threat to the congregation as to its rabbi's imperious leadership. Liberles's account of the events does not substantiate his thesis that Hirsch was "very much on the *defensive*" and that the opposing side "went for broke" and "struck hard at him" (p. 219). Readers who will question his argument here will nonetheless value his well-researched account of the controversy, which corrects many mistaken impressions widely held among historians today.

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REINHARD SCHÜREN. *Staat und ländliche Industrialisierung: Sozialer Wandel in zwei Dörfern einer deutsch-niederländischen Textilgewerberegion 1830–1914*. (Untersuchungen zur Wirtschafts-, Sozial- und Technikgeschichte, number 5.) Dortmund: Gesellschaft für Westfälische Wirtschaftsgeschichte. 1985. Pp. ix, 242.

Reinhard Schüren examines in considerable detail two villages with a population in 1830 of about three thousand each. This study explores the village of Borne in the Netherlands and the village of Borghorst in the Münster region of Prussia. It is a tour de force in which the methods of economic and social history are well used in the face of somewhat sparse data. These two villages are located not far from each other and are subject to physical conditions that are not greatly different.

Besides farming, the textile industry employed the most people. At first, the textile labor force was composed mostly of cottage workers, but later in the nineteenth century they became factory workers, owing to the location of textile mills there. The German village benefited from the protectionist policies of Prussia in the second half of the nineteenth century. The Dutch village, however, was exposed to the free trade policies of the Netherlands. It was, moreover, producing more for an overseas market, whereas the production of the German village went mainly to the Ruhr area. Possibly because of the free trade atmosphere, the Dutch village had one large factory that was expanding throughout the period, whereas the German village had smaller factories in operation. As far as the welfare of these villages was concerned, the German village was increasingly better off than the Dutch village from the 1840s on. Interestingly, the Dutch village was more prosperous before 1840, mainly, according to the author, because the innovation of the fly shuttle on the broad loom had appeared there earlier than in the German village.

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Nominal wages of textile workers in the German village rose from 400 to 500 percent between the 1840s and 1900 while the prices of foodstuffs remained constant. To be sure, rents rose considerably, and coal prices advanced by 100 percent as well. Food was, of course, the most significant expenditure for workers in the nineteenth century, and its price constancy meant that real incomes rose. Because of protectionism in the German area, industrialization progressed there more rapidly than in the Dutch village. The free enterprise attitude in the latter also had the peculiar result that more child labor was employed than in the more restricted German village. In the 1890s, German laws were enacted to protect workers, with the result that the number of hours per workday was reduced.

It is of some interest, too, that mortality rates dropped earlier in the Dutch village than in the German village, but by the 1890s they came closer together, possibly reflecting the closeness of real incomes at that time. The author maintains that the demographic development of the two villages was mirrored by their economic conditions, with prosperity resulting in more marriages and more births. Although this is not a surprising finding, it is interesting nevertheless.

Overall, this comparative study of two villages separated by a national border provides the reader with interesting findings. It is well done, using methods developed in economics and sociology to good effect.

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MITCHELL G. ASH and ULFRIED GEUTER, editors. *Geschichte der deutschen Psychologie im 20. Jahrhundert: Ein Überblick.* (WV Studium, number 128.) Opladen: Westdeutscher. 1985. Pp. 386. DM 29.80.

This collection of essays edited by Mitchell G. Ash and Ulfried Geuter speaks as loudly about the interests of certain younger German psychologists as it does about the history of psychology in twentieth-century Germany. All the authors except Ash are psychologists or social philosophers, and all but two are in their thirties and early forties. Their scientific *Weltanschauung* is that science is no different from the other important institutions of Western life and requires the same scholarly scrutiny that politics, diplomacy, and economics have traditionally received. Many of the contributors also share the revisionist orientation of those New Left critics who do not conceive of science as an automatically progressive and useful enterprise. They want to illuminate the social function of science, and they usually view science as serving primarily the interests of the ruling classes.

The various essays concentrate on academic, experimental, and depth psychology; the editors declare they are consciously excluding developmental and social psychology and the history of psychological thought, although Geuter's chapter on National Socialism belies this last intent. Although the volume includes events of the 1970s, none of the contributors come from the German Democratic Republic because Ash and Geuter believe that no satisfactory research on the history of psychology has been conducted there. Specific subjects covered are the decision of German psychologists to turn to "objective" (empirical) psychology at the turn of the century; the foundering of experimentalism on the criticisms of the philosopher-psychologists as well as on the disillusionments of the scientist-psychologists; psychology and school reform; applied psychology before 1933; psychoanalysis until 1945; the relationship between psychology and the military under the gamut of early twentieth-century regimes (imperial, republican, and National Socialist); the accommodation of psychological theory and National Socialism, as seen especially through the "types" of the Marburg psychologist, Erich Rudolf Jaensch (1883–1940); the postwar Americanization of German psychology; the professionalization of clinical psychology via the fields of counseling, social work, and psychotherapy; and the impact of the radicalized student movements of the 1970s.

By their particular choice of first and last essays, the editors portray twentieth-century German psychologists as being in a state of perpetual "crisis," unable to decide whether their theoretical orientation and research should be relevant to the predicaments of human life. Although various, often conflicting, trends have existed simultaneously in German psychology, it is incorrect to depict this situation as one of eternal "crisis." All of Western psychology has been challenged by changing social and scientific conditions and has responded by discussion and alteration. The position of the essayists points not to the fact of crisis but to their specifically Germanic interpretation of conflict as crisis.

The socially critical orientation of many of the contributors leads to some idiosyncratic emphases. I will cite two examples. Although the authors of the twenty-eight-page whirlwind history of psychoanalysis must inevitably slight much material, they devote over two pages to Wilhelm Reich, the left-wing reformer-activist whose politics were not at all representative of the mainstream of German psychoanalysis. Moreover, although the social and scientific critiques that sprang from the student movement had certain tangible results, the editors and the essayists accord the students an importance beyond their actual impact.

Yet the biases in this volume detract only minimally from Ash and Geuter's fundamentally valu-

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able and sophisticated effort to educate both psychologists and historians to view science as an integral part of society.

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AKIRA HAYASHIMA. *Die Illusion des Sonderfriedens: Deutsche Verständigungspolitik mit Japan im ersten Weltkrieg*. (Studien zur Geschichte des Neunzehnten Jahrhunderts, number 11.) Munich: R. Oldenbourg. 1982. Pp. 215.

This book is the outgrowth of Akira Hayashima's dissertation at the University of Cologne some fifteen years ago, and, as such, it manifests all of the virtues and vices of that genre. Its thesis is that German attempts at a separate peace with Japan during World War I grew out of Germany's efforts to keep Japan neutral in August 1914. These attempts continued during the war because the German military leadership impressed on Germany's civilian leaders that a separate peace would be possible, since after 1915 the war could no longer be won by military means alone and since Japan might well favor such a move, owing to the capture of the German possessions in East Asia and the South Pacific and to the traditional pro-German orientation among many members of the Japanese elite. Japanese imperialism on the Asian mainland would inevitably result in collision with British and American interests in China, and, thus, Germany might well become valuable to Japan.

Hence, many efforts were made to obtain a separate peace, some by enlisting the services of private business leaders such as Albert Ballin, Hugo Stinnes, and Max Warburg, and others originating directly from German officials such as Arthur Zimmerman, the secretary of foreign affairs, and from Paul Hintze, the German ambassador to China. These efforts culminated in a series of "secret" talks held, for the most part, in neutral Stockholm during 1916 between Uchida Sadatsuchi, the Japanese ambassador to Sweden, and his German counterpart, Helmuth von Lucius. What Germany especially wanted in 1916 was Japan's good services to obtain a separate peace between Germany and Russia and to stop the Japanese from making large-scale shipments of weapons and munitions to the Russians. In return, Germany offered only vague suggestions for postwar economic support to Japan and cooperation with Japan's policies toward China, offers that were not of sufficient interest to the Japanese. German policies were destined to fail since they were not based on objective reality and were totally illusory and since the Germans, furthermore, failed to understand Japanese "ambiguity."

This study is painstakingly detailed, making exhaustive use of the copious German sources and, a first, of Ambassador Uchida's dairy. On the other side of the ledger, apart from the frequently convoluted and sometimes turgid prose, is the disturbing unwillingness of the author to use imagination and to make deductions from the material he presents. Japan, after leaking news of these "secret" talks at a time of its own choosing to the Allies in London, Paris, and St. Petersburg, unquestioningly benefited from subsequent treaties with these powers, thereby consolidating its future position in East Asia. And Japan not only used these talks but also was recalcitrant about giving naval aid to the Allies in the Mediterranean and also about applying pressure on Peking regarding China's entry into the war, with the same objective in mind. The author does not make any connection between the talks in Stockholm and Japanese gains in wartime diplomacy, but, to me, the proof of the pudding is in the eating, and to leave matters dangling without arriving at some conclusions represents only a part of the work of a historian.

FRANK W. IKLE
University of New Mexico

ANTHONY PHELAN, editor. *The Weimar Dilemma: Intellectuals in the Weimar Republic*. Dover, N.H.: Manchester University Press. 1985. Pp. 224. $25.00.

Geist versus *Macht*, the rift between argument and action, is in essence the perpetual problem of social and political engagement by intellectuals, and it constitutes the Weimar dilemma. The strength of this book is that it illuminates the complexity of the dilemma for German intellectuals. Since each contributing author develops this theme in a substantive manner, the volume has more thematic coherence than most edited works. Several of the essays, particularly those by the editor, Anthony Phelan, and by Wilfried van der Will and Rob Burns, are also linked by a Gramscian analytical framework.

As Phelan shows in an introductory chapter on Weimar theories of the intellectual, the issue was more than a question of Left or Right or of support for or opposition to the republic. The dilemma remained even for those who made these choices, because it involved the age-old problems of intellectual independence versus commitment, of the integrity of ideas versus institutions of power, and of the relationship between the individual and collectives. Moreover, those Weimar intellectuals who attempted to provide a theoretical framework for synthesizing theory and practice ultimately failed. Kurt Hiller, Hugo von Hofmannsthal, and Karl Mannheim never really reconciled the disparate

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roles of the autonomous, authoritative intellectual and effective political activist.

There were a variety of responses to this dilemma. Keith Bullivant argues that the search for an "ideal synthesis" was a central characteristic of the conservative revolution, a movement that emanated from the unpolitical German idealist tradition and was no match for Nazi brutality. In a related essay the late Richard Hinton Thomas challenges the interpretation that the Nietzschean Ludwig Klages was a Nazi forerunner. A romantic cultural reactionary, Klages viewed power as inherently evil and was pessimistic about Nazism. For some the failure to realize their ideals in the social realm led to suicide. It was, according to Godfrey Carr, "The Golden Age or Nothingness." Others suffered from what Phelan, adopting Walter Benjamin's phrase, calls "Left-wing melancholia." Kurt Tucholsky, for example, tried to be active without aligning with political organizations. He was a victim of self-imposed isolation from power because of his high valuation of the individual and his mistrust of collective judgments; consequently, he remained ineffectual and depressed. A similar failure to transform the world through ideas is analyzed in Stephen Lamb's piece on the Munich *Räterepublik*. The long final essay by van der Will and Burns reflects most clearly this volume's general ideological and political orientation. They contend that only those intellectuals who aligned themselves actively with leftist parties overcame the dilemma. The members of the cultural intelligentsia within the Social Democratic party succeeded by making the collective their creative environment, and Bertolt Brecht achieved a unique synthesis of communist partisanship and artistic autonomy. The authors conclude with a discussion of Gramsci's theory of the "traditional" and "organic" intelligentsia as it relates to Weimar and the Federal Republic.

Those unconverted to this viewpoint will probably find terminology such as "dominant class" and "hegemony" distractive rather than helpful. One might also question the neglect of the Catholic intelligentsia and conservatives unaffiliated with the conservative revolution. But, in general, this volume offers informative, challenging reading, and Thomas makes a major contribution in reinterpreting Klages.

JOSEPH W. BENDERSKY
Virginia Commonwealth University

BRUCE B. FRYE. *Liberal Democrats in the Weimar Republic: The History of the German Democratic Party and the German State Party*. Carbondale: Southern Illinois University Press. 1985. Pp. xv, 295. $24.95.

Of all the political parties of Weimar none has been scrutinized more closely than the left-liberal German Democratic party (DDP). Ready availability of primary source materials obviously has something to do with this. Forty percent of the delegates to the party's conference in December 1919 had doctorates (the term *Bildungsbürgertum* hardly seems adequate for such a phenomenon); hence, the plethora of personal diaries, memoirs, and correspondence, in addition to the extensive party records available to researchers, should come as no surprise. But probably a far more important explanation for the appeal of the DDP, especially to Ph.D. candidates, is the symbolism of the party's history for the fate of Weimar and perhaps for an enlightened liberal humanism as a whole in this century. If Weimar generally served as a forge for our modern sensibility, the DDP, which at some point numbered the Webers (Alfred and Max), Albert Einstein, Friedrich Naumann, Walther Rathenau, C. F. von Siemens, and Theodor Heuss among its members, exuded in a most poignant form all the hopes and all the frustrations that rational and decent men and women are likely to experience in modern society and its politics.

Curiously, despite a host of monographs and articles on individuals and on various aspects of the party, no account of the DDP for the whole of the Weimar period existed until now, apart from the memoir-cum-analysis of Werner Stephan, the former party secretary. Bruce B. Frye has finally filled this prominent gap with a book that reflects his thirty and more years of interest in the politics of Weimar.

The strengths of Frye's volume emanate from his probably unequaled knowledge of the personalities in the party, its organization, the issues that preoccupied it, and the relative importance of various interest groups represented in its ranks. With its measured tone and clear exposition, the book provides an excellent account of the party from within. As such, it is a welcome addition to the growing shelf of volumes devoted to Weimar political parties.

Nonetheless, although useful indeed as a work of reference, the book adds little to our understanding of Weimar or of German liberalism. In fact, its focus is extraordinarily narrow. There is no attempt to place the DDP in the wider context of Weimar politics, let alone in a European or international setting. Given the effort that has gone into this book, the conclusions offered about the eclipse of the DDP are, it must be said, disappointing. A "failure of nerve" is, for instance, listed as a "cause" of decline, and a "passive spirit" is said to have contributed to a "lack of commitment" (p. 203). The logic of such statements escapes this reader. Moreover, the general approach is that of an older Anglo-American liberal historiography on Germany that is quick to

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criticize and moralize and is inclined in the end to emphasize what did not, rather than what did, happen.

MODRIS EKSTEINS
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WILHELM DEIST, editor. *The German Military in the Age of Total War.* Foreword by PAUL KENNEDY. Dover, N.H.: Berg. 1985. Pp. 362. $36.75.

This always solid and, at times, scintillating study is in no sense an official publication of the prestigious Militärgeschichtliches Forschungsamt of Freiburg. Yet the circumstance that the editor and ten of his thirteen associates number among its staff almost gives it that character. Two of the three nonmembers gained their doctorates at the University of Freiburg, and a community of interest may well have grown during their student days.

The foreword by Paul Kennedy and the introduction by Wilhelm Deist present the central thread of the fourteen essays, which reflect a transformation in military history during the last two decades. A focus on war, armed forces, and society has developed apace in Britain and the United States, with notable assists from French military historians. The Freiburg group has been playing a similar role in Germany.

Space forbids even a roll call of the fourteen authors or any systematic review of particular contributions. Emphasis is heaviest on the interwar period, when the sequelae of the First World War had manifested themselves and the coming and character of the Second World War were largely determined. Only a single essay deals purely with imperial Germany; another covers the impact of the two war economies on civilian populations. Five essays concern the period 1919–39; two, the Third Reich generally; and four, aspects of World War II itself.

The first essay dwells largely on the compromise between the army and the bourgeoisie, a compromise conceived to have arisen out of the constitutional crisis of the 1860s. In effect, the army traded support of the social and economic power of the middle class for an extraconstitutional position for itself. In the process, the armed forces, especially the navy, antagonized much of the outside world. Attention is further given to the *Reichsfeinde*, notably, the national minorities, political Catholicism, and the Social Democrats.

The German economic scenes of the two world wars are sharply contrasted. The vastly more favorable picture during World War II is ascribed in part to far better preparations, in part to unbelievably ruthless exploitation of occupied territories.

Separate essays examine the roles of the army, navy, and air force in the rearmament process after 1919. Sharp issue is taken with the widely accepted thesis that the Luftwaffe leaders were irrevocably wedded to the doctrine of ground force support. Despite the lack of four-engine bombers, military leaders had every intention to conduct strategic air operations against civilian populations, with London a primary target. This, of course, could only be attempted after the achievement of bases in the Low Countries. Most of this thinking was based on a gross underestimate of the powers of resistance of the politically divided Western societies.

The navy had no clear idea where it was going. Although happy with the resources now lavished on it, naval leaders were skeptical about Hitler's battleship concepts and had doubts about the subsequent Z-Plan. As it became clear to him that Britain would not turn a blind eye to his eastern expansion, Hitler came to contemplate a showdown with the naval powers.

Much is said about poison gas in military plans and calculations. Far from reconciled with the 1918 defeat, army leaders thought of renewing the war in a few years on the basis of a new chemical warfare potential. Such concepts had a great deal to do with collaboration with the Red Army on the basis of manufacture, storage, and testing on Soviet territory. As late as the spring of 1933, an agreement was concluded on common testing, only to be canceled by Hitler that summer. For a variety of reasons, Hitler's attitude on the use of poison gas was somewhat negative, although he kept his options open until his death.

Essays on the Mediterranean in Hitler's 1940 strategy, on the SS as a military factor, on German military law during World War II, and on the Wehrmacht and the "war of Weltanschauungen" cover much of the German military scene during the conflict. A severe judgment on Wehrmacht leaders holds that, to a greater degree than is often supposed, they were initiated into Hitler's criminal intentions.

Four of the authors collaborate in a concluding essay, "Causes and Preconditions of German Aggression." Hitler's armament program is held to demonstrate the crass Darwinism of his concepts. His unrelenting drive made Germany the strongest military power in Europe by 1939. But the uncontrolled rivalry of the three service branches decreed that available resources were partly wasted.

The Freiburg historians have produced a book of penetrating national soul-searching that is required reading for students of the period. It richly achieves its announced purpose of informing English-speaking readers of the progress of German research in dealing with issues of "war and society."

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GEOFFREY J. GILES. *Students and National Socialism in Germany*. Princeton: Princeton University Press. 1985. Pp. xv, 360. $47.50.

The impact of National Socialism on university students has been studied in a number of monographs published in the last two decades. Geoffrey J. Giles builds on and adds to this literature in a work that examines the example of the University of Hamburg against the background of events in the student movement as a whole.

Giles devotes the bulk of his book to the period after the Nazi seizure of power. He convincingly defends the thesis that the National Socialists were unsuccessful in maintaining their grip on the university students during the Third Reich. For example, the creation of *Kameradschaften* by the National Socialist German Student Union (NSDStB), which were intended to replace the fraternities, affected only a minority of the male students at the universities. Departmental study groups aimed at influencing the disciplines attracted even less support. Interest in the rural labor service flagged noticeably after 1933.

Throughout this work Giles analyzes the failure of the NSDStB to engage more than a comparatively small minority in its efforts. He highlights Oskar Stäbel's decision of February 1934 (supported by Rudolf Hess) to turn the NSDStB into an elite organization and the apparent unwillingness in the party to force students to join the movement in the same way that school children were then being forced to join the Hitler Youth. Giles offers a number of interesting and plausible explanations for this policy, including the residual concern in Germany about the universities' international reputation.

One especially interesting feature of the developments in Hamburg was the presence of Adolf Rein, a historian who became a kind of ideological mentor for the intellectual Right at Hamburg. Rein's own background included youthful enthusiasm for Friedrich Naumann's National Social movement, a dissertation completed under Karl Lamprecht's supportive guidance, and a teaching stint at Syracuse University. By the early 1930s, Rein's position that scholarship ought to serve power and the state had attracted the notice of NSDStB leaders in Hamburg. After the National Socialist seizure of power, the students assisted Rein in realizing his proposal to establish "political faculty groups" at the university. Rein, who was elected rector the following year, proved to be a complex figure who ran afoul of the activist students because of his primary concern for the university's scholarly reputation. A flap over his efforts to award a prize to Ralph Vaughan Williams was one of the factors that led to his loss of the rectorship in 1938.

Giles supplemented his archival research by interviewing a number of the most important student leaders of the period as well as Rein and other Hamburg faculty members. Although the interviews provide a measure of color for the general reader, this work will be of most interest to students of university history and student life before and during the Third Reich.

MICHAEL STEPHEN STEINBERG
Institute of European Studies

WERNER T. ANGRESS. *Generation zwischen Furcht und Hoffnung: Jüdische Jugend im Dritten Reich*. (Hamburger Beiträge zur Sozial- und Zeitgeschichte, Beiheft, number 2.) Hamburg: Christians. 1985. Pp. 180. DM 9.80.

Today Werner T. Angress is a historian of modern Germany living in the United States; some fifty years ago he was a Jewish teenager living under the Nazis. This volume is his scholarly attempt to deal broadly and at a distance with the experiences of his youth.

The first section of his book combines a general survey of those Nazi policies that were directed specifically against Jewish young people with a description of their impact and the responses they evoked. The process of ever-increasing exclusion began in 1933 with the Law against Overcrowding in German Schools and Universities, which set a definite Jewish quota; it continued with psychological pressure and physical abuse exercised on those who remained; and it culminated in total exclusion in 1938. Many of the older children maintained their morale largely through participation in a variety of Jewish youth movements. The authorities tended to favor the Zionist groups because they shared the then-current Nazi goal of speedy and massive emigration. The more fortunate children were able to leave with their families or through emigration programs specifically designed for young people. Of those who remained after Nazi policy for German Jewry shifted from emigration to deportation in October 1941, very few survived. Angress tells this tale briefly but authoritatively, partly on the basis of archival material and the printed memoirs of survivors.

This survey is followed by a case study dealing with a non-Zionist emigration training farm in Silesia called *Auswandererlehrgut Gross-Breesen*. Drawing partly on his earlier study in English that appeared in the *Leo Baeck Institute Year Book* for 1965, Angress here describes and traces the activity of the young people who came to this camp to gain both the firmness of character and the practical skills that would enable them to succeed overseas. Although the author was there himself, he maintains the

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necessary critical stance and uses a variety of sources. While the farm succeeded in providing occupational training and in creating an *esprit de corps*, it was less successful in supplying these highly assimilated young people with a meaningful Jewish identity. Except for one group of about thirty-five that resettled on a farm near Richmond, Virginia, the camp's leadership also failed in its goal of finding opportunities for larger units to remain together. By 1939, they were scattered, mostly individually, on five continents. But those were the lucky ones. In November 1938 the SS had smashed up the camp shortly after *Kristallnacht*, and nearly all of the "third generation" at the farm perished at Auschwitz.

The case study is supplemented by excerpts from a diary and correspondence, published here for the first time. The documents convey the personal reflections of one of the Gross-Breesen young people and of their leader on the Jewish situation and illustrate their frustrating attempt to gain entry visas from abroad. The prevalent inner confusion and turmoil are memorably expressed in a diary entry of November 1938: "It's funny; I'm a Jew and yet not a Jew, a German and yet not a German. I believe in God and then I don't. And that's the way it is with everything. . . . But I want to be whole, so I must find my way out of Europe" (p. 92). Although the documents might have been abbreviated more with little loss, they effectively evoke the mood of an anxious time.

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ROBERT WISTRICH. *Hitler's Apocalypse: Jews and the Nazi Legacy*. New York: St. Martin's. 1985. Pp. viii, 309. $17.95.

The title of Robert Wistrich's book only begins to suggest the disturbing phenomenon that the author seeks to study and explain—the historical connections between Nazi anti-Semitism and contemporary forms of anti-Zionist anti-Semitism. Wistrich believes that "ever since 1933 the war on the Jews which was inaugurated by Nazism has never really stopped" (p. 8). In his first eight chapters he focuses on the causes and consequences of this war during the Hitler period itself. In the four chapters that follow he traces the Nazi "legacy," the latter-day manifestations of anti-Jewish hatred and hostility. Taken together, the book's twelve chapters describe, in closely detailed argument, one of the twentieth century's most persistent and most violent strains of racial and ideological aggression.

Wistrich's early chapters, which go over familiar ground, are devoted to an exposition of Hitler's anti-Semitic obsessions and their development into a politics of anti-Jewish denigration, expulsion, and, ultimately, extermination. Following the lead of Uriel Tal and others, Wistrich understands Nazism as a political religion, one that singled out the Jew, in neo-Manichean terms, as an enemy of cosmic proportions and then projected active opposition to Jews as an ideological tool of domestic and foreign policy. Thus, Wistrich aligns himself with those historians who regard anti-Semitism as a central factor in Hitler's political program and the Final Solution as an essential, and not ancillary, Nazi war aim. The evidence he marshals in support of this view is impressive, and his conclusions, though hardly unqiue, are well drawn and convincing.

It is in the second part of the book that Wistrich's study takes on an aspect of originality and importance that surpasses his synthesizing efforts of the earlier chapters. Anyone even vaguely alert to the rhetoric of "anti-Zionism" should have little trouble recognizing links between the formulations of Arab and Soviet propaganda today and Nazi propaganda of the 1930s and 1940s. Wistrich aims to reveal precisely how these connections have been made and how and where they have been put to practical political use. By and large he succeeds.

His chapter on Arab anti-Semitism, though brief, demonstrates the degree to which political and military leaders within the Arab world have appropriated some of the key texts of Nazi propaganda (*The Protocols of the Elders of Zion*, for instance, circulates in Arabic translation) and have incited anti-Jewish hatred of a racial and religious, as well as a political, nature. While such virulent opposition to Jews and Judaism may not have been indigenous to traditional Islamic society, large parts of the Muslim world now seem receptive to such propaganda. Quoting the Israeli historian Yehoshafat Harkabi, Wistrich notes: "The volume of anti-Semitic literature published in the Arab world has had no parallel in modern history since the demise of Nazi Germany. What makes this literature even more significant is that it has been put out by official government houses—not from the fringes of Arab society but from its very center" (p. 186).

What Wistrich sees within the Arab world he finds with an even greater degree of intensity in the Soviet Union, "the fountainhead of a new antisemitic International" (p. 208). His chapters on Soviet anti-Semitism offer extended analyses of a wide variety of anti-Jewish texts and make distressingly clear that, under the guise of "anti-Zionism," the Soviet Union supports an anti-Semitic campaign of vast proportions, a campaign that, at critical moments, has been used as an instrument of political power in other countries of Eastern Europe and elsewhere around the world. In their relentless efforts to delegitimize Israel as a "successor state" of Nazi

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Germany, the Soviet Union and other centers of anti-Zionist anti-Semitism themselves carry on the work of Nazi Germany and, in so doing, preserve Hitler's legacy for this and future generations.

Wistrich's thesis that we are witnessing a continuing tradition of radical and murderous anti-Jewish passions and political programs is, thus, largely borne out by the evidence presented in this book.

ALVIN H. ROSENFELD
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JOSEF FOSCHEPOTH, editor. *Kalter Krieg und Deutsche Frage: Deutschland im Widerstreit der Mächte 1945–1952*. (Veröffentlichungen des Deutschen Historischen Instituts London, number 16.) Göttingen: Vandenhoeck und Ruprecht. 1985. Pp. 388.

With the opening of the British and some French archives, the historiography of the early cold war and the political settlement of post–World War II Europe has entered a new stage. If the opening of the U.S. archives clarified America's role in the origins of the cold war, the addition of French and especially British sources has directed attention to the multipolarity of interests and conflicts among the four powers in the postwar years.

Clearly no one was more directly affected by this four-power interaction than the Germans. It is thus not surprising that West German scholars have increasingly turned their attention to the formative period of their state and that they have sought a dialogue with British, French, and American colleagues. The volume at hand is a stocktaking of this new German scholarship and related U.S. and West European efforts. The papers by one French, one American, four British, and eleven German political scientists and historians examine such general topics as the German policy of the four powers, their conflicts and cooperation during the occupation, and the role of the new Germany in the emerging East-West conflict.

The individual contributions are skillfully woven together into a comprehensive assessment in Josef Foschepoth's introduction. Underlying the entire project are five important assertions: that the German question and the cold war were so thoroughly intertwined that any comprehensive discussion of one without the other is impossible; that the division of Germany and Europe was ultimately an inevitable result of the search for economic and political consolidation in East and West; that conflicts among the Western allies were often almost as intense as the East-West confrontation; that the junior partners in the Western alliance—namely, Britain—played a far more central role in the postwar settlement and the origins of the cold war than traditionally assumed; and that Germans were not only victims but also protagonists and beneficiaries of the cold war and the division of Europe.

Inevitably some of the individual contributions will be familiar to the reader as summaries (Victor Rothwell on Britain and the cold war; Raymond Poidevin on French policy), revisions (Wilfried Loth on the impact of the Korean War; Rolf Steininger on Stalin's 1952 note) and translations (John Gimbel on conflicts within the U.S. administration) of earlier publications. The freshest interpretations are those based on newly available British and French sources. They stress primarily the active British and French roles and conflicts between the United States and Britain over such issues as food and coal deliveries (John Farquharson), reparations (Foschepoth), and the Berlin blockade (Michael Bell), which found the British far more opposed to general East-West negotiations and determined to safeguard the new West German state than heretofore assumed. Also remarkable are the reassessments of Soviet policy, which conclude that Stalin's "national" German policy was more restricted than generally believed (Dietrich Staritz) but that even in 1952 the Soviets still genuinely considered trading the struggling East German state for a neutral unified Germany (Steininger). It is clear that not only fearful Frenchmen and Britons but also the most important postwar West German leaders were ready to forego national unity almost from the start, because security and prosperity seemed attainable only in a separate West German state.

DIETHELM PROWE
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DAVID HERLIHY and CHRISTIANE KLAPISCH-ZUBER. *Tuscans and Their Families: A Study of the Florentine Catasto of 1427*. (Yale Series in Economic History.) New Haven: Yale University Press. 1985. Pp. xxiv, 404. $32.00.

This abridged translation from the French edition of 1978 presents more attractively a study as monumental as the tax survey of 1427 on which it is based, combining, with Florentine *virtù*, the reverence of the medievalist for his document and that document's imaginative exploitation by modern technology. Over fifteen years ago, David Herlihy and Christiane Klapisch-Zuber began to announce in journals the earliest findings from their computer-processed demographic data. Scanning the Tuscan landscape like the *catasto* that they compare to "the rays of a summer sun" (p. xxiii), these have struck, as a blinding light, scholars treading numerous of those dim paths that crisscross even familiar premodern territory. Thanks to the authors' desire to know the entire population, rich and poor, rural and urban, we now have infinitely more sense of the

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shape and texture of Tuscan society, its inequalities and contrasts, its crises and patterns of change. From their focus on the passage of the household and its members through the life-cycle (birth, marriage, and death), a whole range of questions has been opened up or can now be reformulated—some touching previously inaccessible aspects of the lives of working people, women, and children, others relating to more traditional preoccupations with the governing elite and high culture.

In the lively response—itself a tribute—from a battery of specialists in demography and history, praise has been tempered with suggestions that the reach of this intentionally daring work exceeds its grasp. One is aware of one's own limitations, of the authors' subtle and progressive self-evaluation, and of their not having set out to write *histoire totale*. Yet the inspiring challenge they present to all historians eager to extend their repertoire of approaches to the past deserves to be met head on.

The use of quantitative analysis "to show the range of individual experiences, of choices and constraints, which in the past confronted human beings as they aged" (p. xxiv), seems likely to prove the enduring core of their formidable achievement, and their generous commitment to a machine-readable edition of the *catasto* has made their raw data accessible to all. Their own description of this great document in its political, administrative, and fiscal contexts is masterly. But there remain impediments to the marriage of social "realities" with "values" proposed in the authors' interpretation of experience" (as reflected in the statistical data) in the light of their omnivorous reading of contemporary literature. Closer juxtaposition of words and numbers in this new edition does not bring them "into closer relationship" (p. xxi).

In these pioneering attempts to assess the causes and effects of "demographic deportment," the humane recognition of "the I in thou" that impels the authors to rescue individuals from obliteration by the mass of sheer numbers sometimes bears too much explanatory weight. This delicate task demands more than could reasonably be expected in addition to already herculean statistical labors; it requires more attention to particular dimensions of the social context such as that of public life and the male civic ethos that so affected the lineage of the dominant urban patriciate. It also requires a closer consideration of problems of accident and intention in relation to motivation, of paradox, ambivalence, and contradiction. Placed in a richer cultural context of genre, traditions, and language, words—among them those of the *catasto*—at once prescriptive and descriptive, might mesh more complexly with numbers to describe this world where each literate man, his own ethnographer, ordered and interpreted his experience the more artfully to woo *fortuna*.

The message of written records may be far from clear, and the distinction between boulevards and byways of history less than absolute, but we must admire Herlihy and Klapisch-Zuber for blazing some glorious trails.

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La Trobe University

ROLAND SARTI. *Long Live the Strong: A History of Rural Society in the Apennine Mountains*. Amherst: University of Massachusetts Press. 1985. Pp. xi, 282.

Historians of modern Italy have lagged behind their French and Anglo-American counterparts in exploring society's "grass roots." Indeed, with a few noteworthy exceptions, there has been a marked tendency until recently to identify the working classes of the Italian peninsula with the organized trade union movement. Only those portions of the labor force distinguished by their militancy and political consciousness have attracted much scholarly attention. In this context Roland Sarti's study of the Apennine mountain people of central Italy in the nineteenth and twentieth centuries represents an innovative effort to shed light on a neglected segment of society: peaceful, rural populations that played a seemingly uneventful role and that indulged in gradual, nonviolent change. The author is particularly well suited to such an undertaking. He brings to his study not only the professional skills of a first-rate historian but also knowledge based on the collective memory of his relatives, friends, and *paesani* in the Apennine village of Montefegatesi.

Using the village as his primary reference point, Sarti challenges a number of stereotypes of peasant culture and attitudes. His mountain people displayed no great love for the soil, preferring moderation to extremism and change to tradition when it served local needs. Politically, Sarti characterizes the mountain people as being neither revolutionary nor reactionary but rather "liberal" because of their reliance on voluntary collective action and constitutional freedoms and because of their concern for concrete measures instead of doctrines.

This richly detailed study breaks down roughly into two parts. The first part delineates the nineteenth-century crisis of the old rural order that resulted from population growth, unprosperous agriculture, the loss of common lands, and the lack of nonagricultural employment opportunities. Sarti's mountain people responded with a strategy of adaptation that used their marketable skills, traditions of temporary emigration, and close interaction with urban society. Descriptions of folk customs and

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village rituals are skillfully interwoven into a chronological treatment of these adaptive responses.

The second part looks at the evolution of peasant political attitudes and initiatives after unification. Initially, the mountain population had limited contact with the national state and accordingly were less concerned with national debates than with local taxation, transportation, and education. By the First World War, however, suffrage reform and fear of socialism had brought together Catholics, Liberals, and small landowners to break down the parochialism of village politics. Fascism was imposed by violence and intimidation from outside. Support for the regime came more from identification with Mussolini's personal and political style than from Fascist policies or ideology. During World War II, disillusionment with Fascism coincided with attitudes toward the resistance movement that were surprisingly ambivalent. The liquidation of the Fascist regime and the passing of the old liberal generation magnified the political importance of the parish priests and transformed the mountain people into a bulwark of Christian Democracy.

Sarti's study is not devoid of problems. Occasionally, the very richness of detail obscures the larger themes and issues. Moreover, the book would have benefited from a more analytical conclusion. Nonetheless, Sarti has produced an interesting, innovative, and important contribution to our knowledge of a segment of what might be Italy's "silent majority."

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ABRAHAM J. HESCHEL. *The Circle of the Baal Shem Tov: Studies in Hasidism.* Edited by SAMUEL H. DRESNER. Chicago: University of Chicago Press. 1985. Pp. xlv, 213. $24.95.

The late Abraham J. Heschel, a major scholar and Jewish religious thinker, devoted much of his career to explicating Judaism's understanding of man's relationship to God. In such works as *God in Search of Man: A Philosophy of Judaism* (1955) and *Man Is Not Alone: A Philosophy of Religion* (1951), Heschel attempted to distill the essence of Jewish theology for a wide American and American Jewish audience. In more technical works, many of them written in Hebrew or Yiddish, Heschel explored the origins of Hasidism, a Jewish mystical and pietistic movement born in eighteenth-century Poland that dominated East European Jewish life in the nineteenth and twentieth centuries. In addition to numerous scholarly articles, Heschel wrote a two-volume study of Rabbi Menachem Mendel of Kotsk, and at the time of his early death in 1972 he was preparing a definitive study of Israel Baal Shem Tov, the founder of Hasidism.

In this book Heschel's student Samuel H. Dresner has tried to honor his teacher by presenting four of Heschel's important technical essays on the early Hasidic masters to an English-reading audience. In addition to translating the articles and expanding the scholarly apparatus, Dresner provides a thirty-eight-page essay on Heschel's role in modern scholarship on Hasidism. Dresner argues that, because Heschel, a descendant of several prominent Hasidic rebbes (or charismatic leaders), had familiarity with Hasidism as a living phenomenon, he possessed the necessary qualifications to analyze the movement. Unlike Martin Buber, who romanticized Hasidism in order to fit it into his own Jewish philosophy, or Gershom Scholem, who overlooked the moral and religious message of Hasidism in order to fit it into his typology of Jewish mysticism, Heschel, Dresner insists, truly understood Hasidic texts and leaders. Unfortunately, Dresner laments, Heschel died before he wrote the definitive work on the origins of Hasidism.

Dresner aims here to make Heschel's articles available to the nonspecialist. I fear, however, that these essays are far too technical to be accessible to those without a mastery of Hasidism and of the Kabbalistic tradition on which Hasidism drew. Nevertheless, Dresner has performed an important service to the scholarly community. The four essays in the volume—on the early Hasidic masters Rabbi Pinhas of Korzec, Rabbi Gershon Kutover, Rabbi Nahman of Kosów, and Rabbi Isaac of Drohobycz—reveal that the early Hasidic movement was not as monolithic as Buber or Scholem suggest. Indeed, these four masters diverged markedly from the movement's founder and from his successor, Dov Baer of Mezeritch. Whereas Dov Baer and the Baal Shem Tov stressed enthusiasm and joy and sought to make Hasidism a mass movement, the rabbis studied here stressed the introspective, ascetic, scholarly side of Hasidism. Rabbi Nahman of Kosów, for example, practiced mortifications and fasting and hated this world. These men spent a great deal of time studying Jewish mysticism, and they had different views of the relationship between God and man.

Because these essays are technical, they are by no means as interesting for the nonspecialist to read as Buber's or Scholem's grand expositions on the nature of Hasidism. Moreover, because Heschel himself was not a historian but, rather, a student of religious thought, these articles do not explore the kinds of questions that the historian might ask. They are, however, the necessary building blocks for a fuller understanding of early Hasidic thought. Unfortunately, Heschel himself can no longer provide the larger synthetic work. It is to be hoped that

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scholars such as Dresner and Arthur Green will accomplish this important task.

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MICHAEL G. MÜLLER. *Polen zwischen Preussen und Russland: Souveränitätskrise und Reformpolitik 1736–1752.* (Historische Kommission zu Berlin, number 40; Publikationen zur Geschichte der Deutsch-Polnischen Beziehungen, number 3.) Berlin: Colloquium. 1983. Pp. viii, 275. DM 68.

The downfall of the Polish commonwealth has inspired a distinguished historical literature, to which Michael G. Müller's book is an impressive addition. He focuses on the reform movement during the reign in Poland of Augustus III, the king of Saxony. Between 1736 and 1752 the aristocratic Polish parliament, the nominally sovereign power in the land, seriously debated the crown's proposals to raise new taxes and increase the standing army. Here lay a chance for the "noble democracy" to regain the state power it had lost in the calamitous Northern War of 1700–21, during which Poland had fallen into Russian vassalage. If the opportunities of the reign of Augustus III had been seized, Poland in the 1760s might have confronted Austria, Prussia, and Russia not as a helpless victim at whose expense, in the partition of 1772, the three autocracies might settle their own differences but rather as a state capable of self-defense and secure alliances. Instead, the diets of 1738–52 expired in barren deadlock.

Müller's historiographically well-framed explanation of this outcome constitutes a major reinterpretation of eighteenth-century Polish history. Weighing the question of foreign influence on the commonwealth's domestic affairs, Müller shows that the wars inaugurated in 1740 by Prussia's attack on Austria moved Russia to abandon the Petrine tradition upholding Polish "anarchy" and instead to tolerate Augustus III's reform efforts. Müller convincingly argues that Russia's motive was not to gain a modernized Polish state as a fighting ally but rather to hold Saxony on the Russian side as a counterweight in Central Europe to Prussia, allied now to anti-Russian France.

Prussia's Frederick II opposed Polish reform, but Müller rejects the tendency to blame parliamentary failure on Prussian intrigue. Similarly, turning to the commonwealth's domestic politics, Müller delivers a brilliant analysis of the notorious *liberum veto*, generally regarded as the proximate, if not sufficient, cause of parliamentary paralysis. Müller shows that the *liberum veto* did not allow a single deputy arbitrarily to jam the parliamentary gears, and the *liberum rumpo*, by which parliaments were sometimes dissolved, was widely held to be an unconstitutional abuse. Müller finds that, when decisive majorities commanded the parliament, neither device could thwart them.

The crisis of the commonwealth arose not from its republican constitution but from the fatal equilibrium in which the party of reform (the Czartoryscy), siding with Augustus III and Russia, confronted the republican opposition (the Potoccy), fearful that state modernization would draw Poland into war for Saxon and Russian ends. After 1763, when the crown passed from Saxon back into Polish hands, an era of impressive parliamentary reform ensued for which, as Müller persuasively argues, the debates of 1736–52 were an indispensable rehearsal.

The question remains whether costly reforms were economically feasible under Augustus III. Müller finds that the magnate latifundists, pillars of the republican opposition, saw their already dazzling fortunes improving after the 1720s. But the economic skies of the lesser landlords remained dark at least to mid-century. At the reform diets, it was less the aristocratic selfishness castigated in the historical literature than real constraints on the lower nobility, paired with the political logic of magnate wealth, that doomed the state to penury.

Still, Müller makes a strong case for the primacy of politics, both domestic and foreign, in the twilight of the commonwealth. For the leaders of the reform party, the "family" of the Czartoryskis and Poniatowskis, emerged from the magnate oligarchy to champion enlightened innovations cutting significantly into noble privilege. Müller's excellent book does not attempt to plumb the motives of these admirable figures. Perhaps they are to be found quite simply in the uncorrupted tradition of aristocratic republicanism.

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PÉTER HANÁK. *Ungarn in der Donaumonarchie: Probleme der bürgerlichen Umgestaltung eines Vielvölkerstaates.* (Schriftenreihe des Österreichischen Ost- und Südosteuropa-Instituts, number 10.) Vienna: Geschichte und Politik or R. Oldenbourg, Munich, or Akadémiai Kiadó, Budapest. 1984. Pp. 468.

This book, a joint publication of the Austrian East and Southeast European Institute and the Hungarian Academy of Sciences, gives a thorough historical, social, political, and economic assessment of Hungary between 1848 and 1918. The interpretation follows the current Hungarian version of Marxist-Leninist historiography.

The book consists of two parts: one deals with the period between the revolutions of 1848–49 and the Compromise of 1867, and the second is concerned

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with the era between 1867 and 1918. A separate chapter, which is probably the most interesting part of the book, deals with the changing Hungarian interpretation of the significance of the Compromise of 1867 in Hungarian historiography. Péter Hanák traces the evolving interpretations of the compromise during the half-century existence of the Dual Monarchy. Interestingly, the views of József Révai, the official ideologue of the Rákosi era, attain particular place in this part of the study.

The account of the period 1848–67 is primarily a political history. Although the author does not include much material that is new, he makes adequate use of the recent historical literature. The second part of the book is predominantly social and economic history, but Hanák gives adequate consideration to political and constitutional matters. The basic line of thought in these chapters, as well as in the entire book, is consonant with current official Hungarian historiography, which considers the Compromise of 1867 as the doomsday of modern Hungarian history rather than as the stepping stone of progressive evolution that the Hungarian founding fathers of the compromise—the modern, fair-minded liberal statesmen Ferenc Deák, József Eötvös, and Gyula Andrássy—intended it to be.

Hanák appends a bibliography that is a reprint of the one in *Magyarország története 1890–1918* (1978), edited by Hanák and F. Mucsi, which constitutes part of a ten-volume history of Hungary.

Hanák is one of the foremost students of the modern period of Hungarian history and of the Dual Monarchy in particular. This volume is a detailed account addressed to professional historians, for whom it is interesting reading.

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MARY GLUCK. *Georg Lukács and His Generation, 1900–1918*. Cambridge: Harvard University Press. 1985. Pp. ix, 265. $25.00.

Not content to repeat previous studies of Georg Lukács's circuitous road to Marx, Mary Gluck has painted a scholarly portrait of an entire generation of Hungarian-Jewish intellectuals. She begins by sketching late nineteenth-century Hungary's liberal, if undemocratic, social and political background, highlighting the success of Jewish assimilation. Unfortunately, that achievement proved to be short-lived; challenged by more passionate creeds, Hungarian liberalism soon revealed its vulnerability. By the turn of the century, when Lukács was coming of age, anti-Semitism had already begun to poison the country's public life. As a consequence, recently assimilated Jews became increasingly conscious of their social marginality—no longer Jews, they could not quite be Hungarians. Yet, according to Gluck, it was precisely their experience of alienation that placed these "free-floating" intellectuals in a uniquely advantageous position to observe and participate in European culture, poised as it then was on the brink of the modernist breakthrough.

Gluck supports her argument in the mere act of introducing members of the "Sunday Circle," the remarkable discussion group that gathered around Lukács in 1915. The names of Béla Balázs, Karl Mannheim, Arnold Hauser, Frederick Antal, Charles de Tolnay, and Michael Polanyi will be familiar to students of philosophy, sociology, film theory, and art history. The women in the group—Anna Lesznai, Emma Ritoók, Edit Hajós, and Júlia Láng—are less well known but almost equally talented. Gluck endeavors to portray all of them, women and men, without the aid of historical hindsight, the better to resist the temptation to present bolshevism as the inevitable outcome of their wide-ranging conversations. She does not deny, however, that their "religious" concerns help explain Lukács and Balázs's subsequent conversion to communism. All in all, what she says in this respect is not strikingly original, but that does not detract from the contribution she has made by locating the circle's members within a generational context.

Gluck's effort to deepen our understanding of modernism is more problematic. Lukács and his friends were at odds with materialism and positivism, but they were selective when it came to modernist heroes. They always maintained their distance from *Nyugat* (West), Hungary's leading modernist review, and, although they championed the postimpressionist artists known as The Eight, they were more instinctively drawn to medieval mystics and early Romantics. And if, as Gluck asserts, they were ever aesthetes, it was not for long; although she places their final rejection of aestheticism in 1910–11, Lukács was defending a "metaphysics of tragedy" as early as 1903. But, even if we agree to affix the "modernist" label to the Sunday Circle, Gluck missed an opportunity to search out the roots of the Marxist Lukács's rejection of modernism in his affinity for Thomas Mann, the bourgeois genius to whom she makes only passing reference. This is all the more regrettable because the relationship between Mann and Lukács sheds light on her recognition that Lukács viewed Marxism as the inheritor, rather than the destroyer, of what was best in Western culture.

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SREĆKO M. DŽAJA. *Konfessionalität und Nationalität Bosniens und der Herzegowina: Voremanzipatorische*

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Phase, 1463–1804. (Südosteuropäische Arbeiten, number 80.) Munich: R. Oldenbourg. 1984. Pp. 303. DM 72.

The Ottomans did not, as a matter of policy, forcibly Islamicize the Balkan peoples in the wake of their conquests. Only in Bosnia, in the central region of present-day Yugoslavia, did large numbers of Slavs convert to Islam and subsequently develop a national identity distinct from other South Slavs. Because Bosnia has ever since been inhabited by Catholics, Serbian Orthodox, and Bosnian Muslims, the nature and causes of confessional change are critical to any discussion of nationality.

Srećko M. Džaja's work is an in-depth examination of confessional developments in Bosnia from the Ottoman conquest to the nineteenth century. In his view religion is the key to nationality: he rejects the notion that the Muslims are actually Serbs or Croats of a different religion and thereby sets himself apart from the wide-ranging claims of many nationalist polemicists. In doing so, he oversimplifies the relationship, contending that national identity is a mere refashioning of confessional loyalty in the modern era. That is one of the paradoxes of his book: he presents a fine review of confessional change, but he introduces no conceptual framework to relate such changes to nationality. Despite its title, the work deals only with confessions, not nationality.

Another paradox is the intermingling of clear objectivity with a distinct sympathy for Catholic, particularly Franciscan, viewpoints. The chapter dealing with the Serbian Orthodox is a balanced, objective presentation of the demographic and political changes that influenced the growth of Serbian Orthodoxy. When dealing with the Catholics, however, all but a veneer of objectivity disappears. The author pictures the Catholics as pressured both by Ottoman repression and by Serbian Orthodox proselytism, yet he dismisses Catholic conversion efforts by blaming them on outsiders operating from Dalmatia and Rome. In calling Serbian Orthodox proselytism a "building block of hatred" between Serbs and Croats (p. 209), he sinks to a level of polemical nationalism not present elsewhere in the monograph.

A third paradox is the mixture of an elaborate scholarly apparatus and an almost total absence of new insights. The author is familiar with many obscure tomes and a wide gamut of secondary studies. His work introduces the reader to a complex historical landscape, for he delves into the major controversies of the period armed with an impressive arsenal of background knowledge. He sharply and convincingly attacks the "Bogomil hypothesis" that attributes Islamicization to the conversion en masse by members of the heretical, dualist Bogomil sect. Yet he tends to undervalue the contributions of those who have made similar arguments before him. His alternative explanations for the conversions are tentative and contradictory, and the major points raised in the concluding chapter are often different from those made in the text.

The book is rich in detail and useful as a survey of key historical issues in an area that is not adequately understood. Lacking significant new insights into Islamicization and confessional change, it is best read as an extensive but partisan review of the existing literature.

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ALEXANDRU ZUB. *Biruit-au gîndul: Note despre istorismul românesc* [Thought Has Triumphed: Notes about Romanian Historism]. Iasi: Editura Junimea. 1983. Pp. 382. 20.50 L.

ALEXANDRU ZUB. *De la istoria critică la criticism: Istoriografia română la finele secolului XIX şi începutul secolului XX* [From Critical History to Critical Excess: Romanian Historiography from the End of the Nineteenth Century to the Beginning of the Twentieth Century]. (Biblioteca Istorică, number 65.) Bucharest: Editura Academiei Republicii Socialiste România. 1985. Pp. 311. 32 L.

The study of historiography in Romania has developed remarkably in the last two decades, both in its discovery of the significance of the history of history for understanding Romanian culture and society and in its increasing sophistication and awareness of international historical currents and approaches. From the leading specialists in such studies—Lucian Boia, Pompiliu Teodor, and Alexandru Zub, among others—a wide variety of analyses, both philosophical and interpretive, have appeared in recent years to grace a once-forlorn and nearly bereft landscape.

These two new books confirm Zub's status as the most accomplished and vigorous student of Romanian historiography, past and present. His impressive bibliographical and analytical works on Mihail Kogălniceanu, the founder of modern historical studies in Romania, on A. D. Xenopol, the first great synthesizer of Romanian history, and on Vasile Pârvan, the pioneer of Romanian archaeology, will remain standard for the foreseeable future. His exploration of the historiographical implications of *Junimea*, Romania's leading critical and literary group, recovered the activities of the generation most responsible for setting the stage for the maturation of historical studies in Romania. His study of nineteenth-century Romantic nationalist-activist historiography in Romania, *A scrie şi a face istorie* (To Write and to Make History [1981], neatly and comprehensively captured a key moment in his country's intellectual history while also focusing on a

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significant aspect of East European development as a whole. The two books under review here take this work two steps further.

Biruit-au gîndul is a collection of essays exploring the profound attraction that history and the past have had and still have for Romanians, whether academics or peasants, politicians or poets. This attraction is what Zub labels "Romanian historicism." The title is taken from the seventeenth-century chronicler Miron Costin's justification of his decision to study the national past despite the pressing needs of the present. For Zub, Costin's choice is a paradigm of Romanian culture in general. This is illustrated in essays on Romania's greatest scholars—Dimitrie Cantemir, Kogălniceanu, Titu Maiorescu, B. P. Hașdeu, Xenopol, Nicolae Iorga, Pârvan, Mircea Eliade, among others; on the activism of Romanian historians over the last three centuries; on the use and abuse of the past; and on the continued struggle between "civic passion and intellectual scruple" that has confronted Romanian intellectuals. For Costin and his successors, the dichotomy between passion and scruple was overcome by the myriad linkages between national identity, survival, and advance on the one hand and the study of the past and other cultural occupations on the other hand.

De la istoria critică la criticism traces the development of Romanian historiography from the Romantic era of the mid-nineteenth century through the emergence of professional historical scholarship at the end of that century to the appearance of the critical "New School" in the 1930s. It is, in effect, a continuation of *A scrie și a face istorie* into the interwar era. Zub focuses his narrative on the work of what he argues are the three formative giants of Romanian historiography: Loan Bogdan, Dimitrie Onciul, and Iorga. These three wrested the study of history at the Romanian universities, at the academy, and in cultural institutions from the hands of the genealogists and antiquarians and passed the discipline into the keeping of a new generation of professional historians.

After the First World War, the critical approach evolved into criticism for its own sake, which Zub feels became "critical excess." Iorga, once the slayer of the giants of the Romantic movement, himself became the target of outrageous slings and arrows from another, new generation, the Giurescu-Panaitescu-Brătianu group. Nevertheless, the confrontation was evidence that the critical current had triumphed in Romanian historical scholarship and that the gap that had existed between Romanian and European historiography had been narrowed to insignificance.

The "hero" of the account is Iorga, whose mind-boggling production (twelve hundred books; twenty-five thousand articles), polymath scholarship, and protean personality brought him world renown. Iorga was active on every front: he was a prodigious collector of documents, a caustic critic of historiographical romanticism and mediocrity, a promoter of regional and worldwide perspectives and cooperation. For Zub, Iorga's combination of the methods and exacting standards of the critical trend with the color, scope, and vivid sense of drama of the Romantic trend was the ideal synthesis and strategy.

This book is a masterpiece of its genre. It provides for the first time an in-depth analysis of and a context for an era in which most of the leading Romanian historical scholars and scholarship emerged. The previous absence of such an analysis accounts in part for the relative lack of attention that modern historiography had in Romania until the 1970s. The lack of such a context accounts for the nonappearance to date of comprehensive studies of such pivotal figures in the Romanian cultural and intellectual history as Bogdan and Iorga. On balance, the picture drawn of Iorga and this era will be of immense value for all subsequent study. The vignettes of Xenopol, Pârvan, Bogdan, and Onciul are also convincing. The critique of the interwar opponents of Iorga as "hypercritics" is less persuasive. Apart from a certain lèse majesté, the offensive and work of C. C. Giurescu and his colleagues were timely, telling, and salutary.

These two books are solid evidence of Zub's place in the first rank among scholars in his own country and in Europe. He is equally at home in the historiographical milieu of pre–World War II Europe, in that of his own people, in the evolution of cultural institutions, and in analyzing the subtle interplay of personality, conviction, and circumstance necessary to his story. His documentation is thorough and often astonishing in scope (the inclusion of comprehensive indexes in both books makes it that much more accessible and useful), yet his works are not the usual plodding through classic texts and bibliography. They are a credit to the national historiography from which they emerge. It is to be hoped that translations of these and comparable works will make such scholarship more generally available.

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ARCADIUS KAHAN. *The Plow, the Hammer, and the Knout: An Economic History of Eighteenth-Century Russia.* Assisted by RICHARD HELLIE. Chicago: University of Chicago Press. 1985. Pp. xi, 399. $65.00.

The preface to this study of the economy of eighteenth-century Russia explains that the manuscript was finished and accepted for publication before Arcadius Kahan's unexpected death in February 1982. Richard Hellie, the author's colleague at the

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University of Chicago, edited the completed manuscript, wrote a brief conclusion, and, with some assistance, compiled the bibliography. As published, the book is useful, informative, but uneven and in many ways unsatisfying.

In nine chapters the book covers the major elements of eighteenth-century Russian economic history: population growth, agriculture, industry, foreign trade, internal trade, transportation, banking and credit, the fiscal system, and the political order. In its breadth and scope this book provides a more complete and comprehensive picture of the eighteenth-century Russian economy than can be found in any comparable work. Its hundreds of statistical tables, gathered from a variety of published sources and presented in a clear and useful manner in a single volume, make this a valuable reference for scholars seeking statistical information about the Russian economy in the eighteenth century. For the most part the book shows the author's considerable skill in correlating data from different sources, interpreting trends that emerge from the figures, and explaining what the statistics reveal about the Russian economy.

The individual chapters and even sections within chapters vary greatly in their depth and quality. Although not without shortcomings, the chapters on agriculture, industry, and foreign trade, for example, cover their subjects adequately and make a substantial contribution to our knowledge and understanding. At the other extreme, the chapter on transportation devotes most of its attention to overseas and intercoastal shipping and to the conditions of employment on the inland waterways, but it says little about the economics of transportation along those waterways and almost nothing about transportation overland. Much of the chapter on population growth is an unsatisfactory muddle of disparate facts and observations, as the author virtually conceded in its last paragraph, on page 44.

The unevenness of the text reflects the strengths and weaknesses of the bibliography. The great majority of the works cited there and in the notes were published in the Soviet Union between 1956 and 1980, and, where the author drew on works such as S. G. Strumilin's study of metallurgical enterprises, E. I. Indova's studies of agriculture, and B. N. Mironov's study of grain exports, he made critical, discerning use of their findings. In many other instances the author simply did not seek out the information he needed. The chapter on transportation is a case in point. Although the bibliography includes a book on inland waterways by E. G. Istomina published in 1982, one wonders how the author could have read it, much less made use of it, before his death in February of that year and finds, reasonably enough, that the chapter on transportation shows no knowledge of it. Much less reasonably, the chapter shows no knowledge of Istomina's previously published article on waterways or of several comprehensive works on that subject published in Russia in the nineteenth and early twentieth centuries. Furthermore, the author did not make appropriate use of Bernd Knabbe's 1975 publication, *Die Struktur der russischen Posadgemeinden*, in *Forschungen zur Osteuropäischen Geschichte* (vol. 22), which contains much useful information on roads and overland transport.

Similar problems affect many other parts of the book. Recent publications in a rapidly advancing field have already pushed the limits of our knowledge far beyond the point at which they stood when Kahan completed his manuscript. In many other instances significant, disappointing gaps in Kahan's work could have been filled by works of non-Soviet scholarship that the author failed to consult. That is particularly true in the case of dissertations and articles by George Munro, Wallace Daniel, Wilson Augustine, David MacMillan, Denise Eeckaute, John P. Ledonne, and others, many of which are based on materials, including Soviet archives, that were not available to Kahan. In another instance, Jake V. T. Knoppers's three-volume study of Dutch trade with Russia is used in the section on shipping but not in the earlier chapter on foreign trade, which suffers from its absence.

The text contains a number of unsupported assertions as well as numerous digressions and pointless comments that the editors would have done better to delete. The brief conclusion written by Hellie provides a clear, succinct summary of the author's findings and conclusions.

Anyone interested in Russian economic history of the eighteenth century should begin with this book. Hellie and the University of Chicago Press deserve our thanks for seeing to its publication.

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A. I. IUKHT. *Gosudarstvennaia deiatel'nost' V. N. Tatishcheva v 20-kh–nachale 30-kh godov XVIII v.* [The State Activities of V. N. Tatishchev from the 1720s to the Beginning of the 1730s]. Moscow: Nauka. 1985. Pp. 367. 1 r. 70 k.

V. N. Tatishchev is best remembered for his multivolume history of Russia and for his role in creating the Russian Academy of Sciences. Yet Tatishchev spent most of his life engaged primarily in economic and fiscal service to the state, and it is this service, particularly during the turbulent 1720s and 1730s, that constitutes the focus of A. I. Iukht's interesting investigation.

This book can be said to have both an economic text and a political subtext, and it is uncertain which

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is of greater interest. Iukht's narrative describes in successive chapters Tatishchev's involvement in the development of mining in the Urals and Siberia, his trip to Sweden during the mid-1720s to study mining technology and finance, his responsibilities for stabilizing currency and increasing the value and circulation of coinage after its debasement during the reign of Peter the Great, and his participation in the succession crisis of 1730.

Each chapter provides considerable detail about Tatishchev's activities, his goals, and his at best mixed successes. One even gains a glimpse of Tatishchev's personality—quite unusual for a Soviet account—from descriptions of confrontations with powerful individuals, entanglements in financial scandals, and Tatishchev's emergence from these with his reputation only slightly tarnished. In the process Iukht provides excellent summaries, based largely on original archival research, of the state of Russian mining and coinage at the time.

Iukht is at his best, however, when he derives political and ideological meaning from Tatishchev's behavior and from his stands on important issues. He criticizes equally Soviet specialists—such as N. I. Pavlenko, G. A. Protasov, and especially A. G. Kuz'min, who sees Tatishchev as a progressive opponent of autocracy—and what he takes to be a tendency of bourgeois historiography to make Tatishchev into merely a man of state, devoid of class allegiances. Yet, as Iukht makes clear, Tatishchev's various allegiances fit together imperfectly at best. Tatishchev repeatedly demonstrated his complete loyalty to the crown, yet he composed a statement, apparently shortly after the succession crisis of 1730, that suggested some formal limitations on autocratic power. Iukht identifies Tatishchev with the interests of the gentry (*shliakhetstvo, dvorianstvo*) over those of the aristocracy (*boiarstvo, verkhovniki*) and factory owners. Yet Tatishchev believed in economic development, whether private or state-managed, a commitment that Iukht sees as an endorsement of capital and the forces of economic productivity.

Iukht concludes that Tatishchev's efforts to combine what were fundamentally irreconcilable interests demonstrated sophistication rather than foolishness because they exhibited a sensitivity to the complexity of objective conditions: the political stalemate among conflicting social elites—a product of Russia's elongated transition from feudalism to capitalism—that resulted in the state having a relatively free hand during the eighteenth century. Tatishchev, then, was something of a visionary who nevertheless could not resolve all of the contradictions that he perceived.

Although the argument is rendered in an unduly schematic fashion, this is more a reflection of Soviet historiographic convention than of Iukht's own scholarship, which is marked by an admirable sobriety and attention to detail. One only wishes that he had a greater familiarity with recent Western scholarship, in particular the works of Brenda Meehan-Waters, David Ransel, and Arcadius Kahan, whose concerns touch his directly. A more extensive review of Tatishchev's historical thoughts and pedagogical activities, moreover, could have amplified several of Iukht's arguments. But these are relatively minor criticisms of what is an important addition to the literature on post-Petrine Russia.

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O. A. GOLINENKO *et al.*, editors. *The Diaries of Sophia Tolstoy*. Translated by CATHY PORTER. Foreword by R. F. CHRISTIAN. New York: Random House. 1985. Pp. xxxv, 1043. $35.00.

Sophia Tolstoi has had a bad press. Viewed through the eyes of Leo Tolstoi, his disciples, and his admirers, she has been labeled selfish, materialistic, neurotic, nagging, and shrewish. The influence of women's studies, however, has motivated people to reconsider. Anne Edward's book, *Sonya: The Life of Countess Tolstoi* (1981), takes Sophia's side, and the publication of her diaries enables her to speak for herself.

Based on the two-volume Soviet abridged edition of 1978, the diary covers the forty-eight years of the marriage, from 1862 to 1910. In addition to the main diary, selections from Sophia's daily diary (brief entries that supplement or substitute for the main diary) are included. Other editions and English translations of parts of her diary have been published, but the 1978 version, edited by O. A. Golinenko *et al.* and recently translated by Cathy Porter, is still the most complete. Some of the cuts made in the first Soviet edition (1928–36) have been restored, but other material has been deleted, and this is not noted in the text.

Both Leo and Sophia Tolstoi kept diaries for the other to read. Leo Tolstoi conceived the idea, which he believed would foster a completely honest and open marriage. Whether it made for a better marriage is another matter. Before the wedding he had Sophia read his diary with its revelations of multitudinous sexual liasons and an illegitimate child. She never got over the shock. As the marriage developed, and deteriorated, her diary, as well as his, became a weapon to please, cajole, or wound the other. The happy moments are recorded briefly, if at all, whereas page after page, especially after 1881, document her loneliness, anxiety, and increasing desperation, including thoughts of suicide.

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Tolstoi emerges as a cold, egocentric hypocrite who did not practice the marital celibacy he preached. As a husband, he was insensitive and neglectful to the point of cruelty. He forced his wife to continue breastfeeding their first child despite the excruciating pain it caused her, and, when she stopped on doctor's orders, he refused to speak to her. When the onset of labor pains (before the birth of their daughter Sasha) prevented her from copying out one of his manuscripts, he was furious. "He is incapable of loving," she wrote; "he never learned to when he was young" (p. 89). He himself said, "there is no such thing as love, *only the physical need for intercourse and the practical need for a life companion*" (p. 93). He was jealous when she developed other friendships and interests, yet he left her alone for hours on end. As a father, he was indifferent to his children and particularly cruel to his sons. After 1885, under the influence of the insidious and opportunistic Chertkov, he even tried to disinherit them. Sophia, of course, tried to protect her children; continuing battles over finances were a major cause of Tolstoi's departure from Iasnaia Poliana on November 9, 1910.

The pressures on Sophia were enormous. She had all the responsibility for running the estate and the household, and she played the "heavy" to the peasants, the servants, and the children, which she deeply resented. "He has foisted on me the role of *scourge*," she complained (p. 74). At the same time, she copied out Tolstoi's manuscripts and was his first critic and editor, but this was a task she liked, for she prided herself on her influence on his work. Loyal and devoted to him, she had a personal audience with the tsar to plead for permission to publish "The Kreutzer Sonata," despite her hatred of the work itself.

Was Sophia a victim or a martyr? It is clear that her husband was the center of her life and that she remained deeply in love with him to the end. From the very beginning of their marriage she accepted his idea of her role as the "nursemaid of talent" (p. 41). Even in 1903, after two decades of suffering, she wrote, "I must *understand* and *remember* that his purpose in life is to *teach* people, to *write*, and to *preach*. His life, our life, the lives of everyone in his family must serve this purpose. . . . One simply has to close one's eyes to all the compromises, discrepancies, and contradictions and see Lev Nikolaevich as a great writer, preacher, and teacher" (pp. 470–71).

Of obvious importance to historians of Russia and to lovers of literature generally, Sophia Tolstoi's diary should also be read by those interested in psychohistory and psychology. George Orwell once wrote (in his essay on Gandhi), "Saints should always be judged guilty until they are proven innocent." Orwell had in mind the moral bullying practiced by people such as Gandhi (who was influenced by Tolstoi), their almost inhuman detachment from their wives and children, and their tendency, in his view, to exchange one form of egoism for another. Many of Tolstoi's contemporaries viewed him as a saint-like figure, an exemplar of the Christian virtue of altruism, and, in this respect, a contrast to Nietzschean individualism. Sophia Tolstoi's diary, despite its partisan and subjective nature, is eloquent testimony to what it is like actually to live with a saint.

Porter's translation reads well. Her introduction, that of R. F. Christian (editor of Leo Tolstoi's diaries), and the synopsis of important events in Russia and in the lives of the Tolstoi family give helpful background for those who are not experts in the field.

BERNICE GLATZER ROSENTHAL
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STEPHEN M. BERK. *Year of Crisis, Year of Hope: Russian Jewry and the Pogroms of 1881–1882*. (Contributions in Ethnic Studies, number 11.) Westport, Conn.: Greenwood. 1985. Pp. xvi, 231. $39.95.

The wave of pogroms that broke across southern Russia in 1881–82 was the decisive event in the history of Russian Jewry in the nineteenth century. The pogroms began to disillusion the Russified Jewish intelligentsia with the prospects for integration into Russian society, expedited the rise of Zionism and Jewish varieties of socialism, and triggered the first large-scale Jewish emigration from Russia. The frightening eruption of mass violence directed the imperial government into the politics of despair, represented by the abandonment of any hope in the resolution of the Jewish question through integration, assimilation, or socioeconomic reform. The Russian state henceforth pursued policies based on a fear of the Jews as a negative political, social, and economic force, whose activities were likely to provoke outbursts of popular wrath.

Such dramatic events were quickly overgrown by myths that persist to this day in general surveys of Russian history. There has long been a need for a book that would synthesize the work of Hans Rogger, I. Michael Aronson, M. Mishkinsky, Y. Slutsky, Y. Maor, N. Gelber, C. S. Ingerflom, and others.

Stephen M. Berk's book partially accomplishes this task. The emphasis of the book lies in the impact of the pogroms on the Jewish community. It does not match the scale or depth of Jonathan Frankel's recent *Prophecy and Politics* (1981), and such an approach precludes a fully comprehensive account. Thus, while many old shibboleths are challenged, nothing is put in their place. Berk ably summarizes what did not happen—that the pogroms were nei-

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ther started nor maintained by the Russian government, the Holy Retinue, or the revolutionary movement—but he offers only random conjectures as to what actually did occur.

Berk's attempt to place the pogroms in a wider, Russian context is unsatisfactory. His general survey of the rising tide of Judeophobia among the Russian intelligentsia and state officials helps explain their response to the pogroms but not the pogroms themselves. The best source for the pogroms is not the Russian Jewish press, as he claims (p. 217), but the Russian press as a whole, since the Jewish press had its own prejudices and preconceptions. A reading of the Russian newspapers of the period available in the West, for example, would reveal that M. N. Katkov and his *Moskovskie vedomosti* did not remain "silent through 1881" (p. 70) but propagated almost single-handedly the view that the revolutionary movement was responsible for the disorders. The oft-repeated claim that there was an anti-Semitic "press orgy" blaming the Jews for the assassination of Alexander II and emphasizing the role of Gessia Gelfman (pp. 54–55) is, quite simply, a myth.

Pogroms were not new to Russia, having occurred in Odessa in 1821, 1849, 1859, and 1871 without press agitation. Pogroms were not a purely "Jewish" phenomenon: a pogrom against Muslim workers in Baku in 1881 was contemporaneous with the anti-Jewish pogroms. Neither urban nor rural violence was unusual in Russia, where there was a long tradition of "golden charters" that called on peasants to seize the landlords' land or "beat the Yids." Furthermore, Berk misreads some of the evidence. The pogroms did have a large element of urban, petty bourgeois rioters, but the rural, peasant element should not be overlooked. It should be recalled that the "barefoot brigade" of urban rioters was made up not of migrant workers but of migrant peasants looking for work. The book also contains a few casual errors of fact: the Russo-Turkish war took place in 1877–78, not 1873–74 (pp. 48–49).

Berk has cleared away much of the undergrowth of legend that surrounds the events of 1881–82, and he has produced a literate and readable general survey. But a definitive description of the "southern storms," especially one that will place them in a Russian context, remains to be written.

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A. M. ANFIMOV. *Ekonomicheskoe polozhenie i klassovaia bor'ba krest'ian evropeiskoi Rossii, 1881–1904 gg.* [The Economic Status and the Class Struggle of the Peasants of European Russia, 1881–1904]. Moscow: Nauka. 1984. Pp. 230. 2 r.

This book is a companion to A. M. Anfimov's *Krest'ianskoe khoziaistvo evropeiskoi Rossii, 1881–1904* (The Peasant Economy in European Russia, 1881–1904 [1980]), which focuses on the agricultural aspects of the peasants' economic life. In these two studies Anfimov seeks to present a comprehensive survey of the peasantry on the eve of the 1905 revolution.

The purpose of the more recent book is to demonstrate the contradictory nature of the peasant economy, which, on the one hand, was progressive and in the process of transition to monetary—though not necessarily capitalist—relations and, on the other, was held back by the survival of noble landownership. To this end, the author provides a valuable survey of the nonagricultural aspects of the peasant economy, including the various sources of off-farm income, the role of taxation and other obligatory payments and responsibilities, and the peasant movement. One of Anfimov's most interesting observations is that the role of nonagricultural earnings had become so important that neither the area of land sown nor the number of working animals per household is a sufficient measure of peasant well-being. Indeed, in the non–black earth provinces of the industrial north, agriculture had effectively been transformed into a subsidiary occupation. Overall, almost half of all peasant households in the south and three-quarters of those in the north (drawn from all socioeconomic strata and including perhaps 50 percent of the adult male population) were involved in off-farm work for at least some portion of the year, whether as agricultural laborers, industrial workers, rural craftsmen, or operators of their own enterprises.

Despite these signs of economic development, there was a dearth of employment opportunities, particularly in the southern black earth region. Even in the north, overpopulation and underemployment left the equivalent of one worker in eight idle. In addition, many noble landowners depended on such exploitative, though declining, practices as winter hiring and labor renting. Peasants were also subject to heavy taxes. To determine just how heavy, Anfimov examines their obligations to state, zemstvo, and commune. To these, however, he also adds the cost of renting and purchasing additional land to compensate for land hunger. Here, too, the market exercised its influence, driving up land prices until they conformed to the capitalized value of the land's net income. Rents grew more slowly, however, and even declined in relation to purchase prices.

Altogether, Anfimov estimates that average gross annual income from agriculture was thirty rubles per capita, 30 percent of which went to obligatory payments. He does not, however, include the all-important nonagricultural earnings. Moreover, he

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seeks to counterbalance this not unfavorable picture with evidence of continued crop failures and the growth of food supply loans, although these only absorbed less than one additional ruble of income. Then, in a further effort to underline peasant exploitation, he cites a budget study from Kaluga province in the north, where obligations averaged 128 percent of net "agricultural" income. But the picture changes markedly if nonagricultural earnings are included, as the author himself earlier counseled. Following the same line of argument, Anfimov also claims that the picture of peasant differentiation is not substantially modified by taking family size into account. The Kaluga study, however, does not support his argument.

Despite these criticisms, the reader will find a wealth of information in this book. Regrettably, it does not help resolve the current debate among Western scholars as to whether the condition of the peasantry was improving. Nor does it question whether arrears were indicative of increasing impoverishment, although there is an interesting discussion of the government's relatively lenient collection procedures.

DAVID A. J. MACEY
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L. M. DAMESHEK. *Iasachnaia politika tsarizma v Sibiri v XIX–nachale XX veka* [Tsarist Tribute Policies in Siberia from the Nineteenth to the Beginning of the Twentieth Century]. Irkutsk: Irkutsk University Press. 1983. Pp. 133. 1 r. 30 k.

Iasak is a Siberian term for the tribute that the Siberian nomadic and migratory peoples gave to their overlords. It was compulsory and established by law, thus different from voluntary offerings. After the conquest of Siberia *iasak* went to the Russians. It was usually furs and was for the use of the "cabinet," that is, for the Romanov household.

L. M. Dameshek has limited his study to the period when Siberia was integrated into the Russian empire, when systematic efforts were made to convert the pagan Siberian peoples into settled, and Christian, agriculturists. He seems concerned not only with describing and analyzing the imperial Russian government's tribute policy but also with making it fit Lenin's remarks about feudal land rents. Even so, the book has considerable merit.

Using published materials and archival sources in Siberian repositories as well as in the Central State Historical Archives, Dameshek has clarified how the desires of the imperial household for fine furs and *iasak* income conflicted with the policies of getting the aborigines to settle down. Settled agriculturists were freed from *iasak* and, in any case, were in no position to furnish fine furs, but the treasury received twice as much in tax from settled peasants as from nomads.

Another problem with *iasak* was the unrealistically low value set on furs by the cabinet, so that native hunters and trappers found it profitable to sell the furs privately. Also, unscrupulous officials often victimized the aborigines by demanding more tribute than the law required and then by pocketing the balance. The vast distances and limited administrative resources thwarted governmental efforts to prevent private sale of furs and abuses by its own officials.

Eventually the cabinet reluctantly concluded it was necessary to let the treasury collect *iasak* in monetary form, pay the funds into the treasury, then transfer the appropriate amount to the cabinet account. This policy was introduced slowly and was not yet fully in effect when the events of 1917 ended tribute collection altogether.

Dameshek's book would have been more interesting had he referred more often to specific tribal groups. Instead, he usually lumps them all together as "aborigines" (*inorodtsy*), the official term for non-Russians. Furthermore, Dameshek's Russian is pretentious and somewhat stilted.

Still, the account of the conflict between Romanov interests and state policies, and of the way they were reconciled, will be useful to any student of Siberian development and to anyone interested in Russian nationality policies.

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DIETRICH BEYRAU. *Militär und Gesellschaft im vorrevolutionären Russland*. (Beiträge zur Geschichte Osteuropas, number 15.) Cologne: Böhlau. 1984. Pp. x, 504. DM 138.

This is a welcome study in a crucial area that until recently has suffered undue neglect at the hands of Russian historians. In a treatment that begs elaboration at nearly every turn, Dietrich Beyrau has attempted to discern the social, political, and economic dimensions and implications of military development within the broader framework of an evolving authoritarian political system. Given the ambitious nature of his objectives and the limitations of sources, Beyrau has succeeded admirably in producing a study that strikes a balanced approach between the preoccupations of traditional military history and the concerns of contemporary military sociology.

The result is an account as rich in anomaly and contradiction as the historic controversy between Slavophiles and Westernizers. On the one hand, reasons of employment, deployment, structure,

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technology, and support decreed that the imperial Russian army take the well-worn path to institutional modernity followed by other European armies. On the other hand, Russian circumstances always and everywhere imposed constraints and dictated adaptation. Consequently, as Beyrau points out, we are left (for more reasons than merely cultural ones) with an odd hybrid, an organization that was simultaneously of and outside its parent society. Incongruity and discrepancy fed a series of tensions, the nature and implications of which the author painstakingly traces through two sets of major military and related reforms between 1700 and 1880.

For seasoned observers of Russian society, this book's conclusions have a familiar and unsettling ring. Here are military institutions that the social and economic infrastructure never fully sustained, that both supported and imprisoned the monarchy, that both fostered and retarded other aspects of development, and that both required and arrested the growth of military professionalism. Indeed, for the military, Beyrau neatly sums up the maddening implications of these and other anomalies on the eve of World War I by highlighting the plight of the army's professional elite, the *genshtabisty*. The members of this elite managed to survive the tribulations of the period between 1905 and 1914, but lapses beyond their control and larger institutional failures later rendered them "generals without an army."

Beyrau modestly labels his study "preliminary," but he has done much to identify the territory, stake out the ground, and begin an exploration of its main contours. His work reveals an impressive command of the primary and secondary materials, and for this reason alone it will serve along with the pioneering studies of William Fuller, John L. H. Keep, Jacob W. Kipp, and Walter M. Pintner as a major point of departure for further examination of this most important topic.

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ROBERT SERVICE. *Lenin: A Political Life*. Volume 1, *The Strengths of Contradiction*. Bloomington: Indiana University Press. 1985. Pp. x, 246. $24.95.

The first in a series of three volumes that Robert Service plans on the life of Lenin deals with the period from Vladimir Ulianov's birth through January 1910, a point selected as a low ebb in that leader's political fortunes. The number of volumes in this plan does not, however, imply that the work when complete will be notable for its monumental bulk. On the contrary, this portion of the study consists of under two hundred pages of text, and Service clearly has worked hard to compose short chapters studded with short, direct sentences. His success in executing this approach is one of the great strengths of the book, which condenses an impressive array of materials, especially primary sources, into a lucid, compact biography, accessible to undergraduates and rewarding for professionals.

"Life" in the title appears to refer both to Lenin and to the present work about him. Service sees him as a man wholly given to politics and therefore a fit subject for a book that concentrates overwhelmingly on Lenin's political career, making only sketchy references to his personality. Thus, Lenin's first twenty-five years pass in about fifty pages, permitting Service to buckle down to his interpretation of Lenin's career in Marxist theory and Social-Democratic politics. The main narrative line reveals no surprises. The St. Petersburg underground, *Iskra*, *What Is To Be Done?*, the split between Bolsheviks and Mensheviks, the revolution of 1905, the establishment of the Duma, and factional difficulties in the postrevolutionary reaction are all highlighted. The strength of the work lies rather in its clarity of presentation and thoughtful interpretation. One notable theme is that Lenin was a species of orthodox Marxist rather than a populist or Jacobin. Service sees him as a many-sided leader, one as capable of being reasonable and flexible as of being intemperate and stubborn.

But the book pays a price for its compression of complex topics. For example, Service notes that some scholars maintain that in 1903 Lenin and Martov did not understand the broad implications of their differences concerning the definition of party membership. Service disagrees, and may be right, but his few words on the matter are little more than assertion. Likewise, he remarks that Lenin's "perception of the possibilities open to the autocracy continues to offer valuable guidance to historians" (p. 162). It is unclear to me what this means, although it might refer to the immediately following mention of Lenin's argument that the Stolypin agrarian reform represented the "Prussian" rather than the "American" model. But is this not a keen polemical device rather than a historical insight? Surely the main point of the reform was the encouragement of the American model. Lenin himself expressed concern that this might succeed enough to undercut the chances of revolution. In fact, this largely accounts for his zeal to discredit P. A. Stolypin with the tendentious argument that the reform was basically an attempt to help the landowning nobility.

What is remarkable, however, is not that such problems exist in a concise book on Lenin but that

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Service has managed to pack so much into such a portable work.

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ABBOTT GLEASON *et al.*, editors. *Bolshevik Culture: Experiment and Order in the Russian Revolution.* (Studies of the Kennan Institute for Advanced Russian Studies, the Wilson Center, number 5.) Bloomington: Indiana University Press. 1985. Pp. xii, 304. $27.50.

This volume presents a richly resonant collection of articles on the transformation of Russian into Soviet culture, a process initiated by the fall of tsarism and by the October revolution. This break with the past, seen in the historical perspective of more than sixty years, created institutions and a social order that were not as novel and unprecedented as they seemed at the time.

The sixteen essays comprising the book start with Richard Stites's suggestive essay on the interplay of iconoclastic tendencies in the revolutionary upheaval and the conservative preferences (as well as policies) of those in power, juxtaposing the popular looting to the official need for a "cultural order." Robert C. Tucker examines Lenin's concept of cultural revolution and its emphasis on "consciousness" having to overcome popular "spontaneity" through a long-range process of pedagogical effort. Sheila Fitzpatrick covers the impact of the civil war on the formation of centralized rule and on the emergency mentality that in the long run put an end to the vigorously varied cultural life that emerged from the October revolution. Nina Tumarkin supplements this thesis with a description of the founding of Lenin's cult during the same period. David Javorsky pursues the theme of "fortress mentality" that inevitably accompanied attempts to create a new culture based on the "scientific" principles of Marxism.

Several essays treat more specific aspects of the "new" culture. James McClelland examines the educational goals of the Communist regime. Peter Kenez describes Lenin's position on freedom of the press, and Jeffrey Brooks presents his findings on the destruction and the re-creation of the printing industry. Katerina Clark deals with the rise of peasant literature, which she sees as symbolic of utopian, anti-urban trends. Richard Taylor traces the birth of Soviet cinema, and John Bowlt examines the utopian and the practical aspects of constructivist fashion design.

Social aspects of the new Soviet culture are covered in three essays. Barbara Clement writes on the birth of the new Soviet woman, Beatrice Fansworth on the actual experience of the village women, and Peter Juviler on juvenile crime and rehabilitation.

The multilayered treatment of Bolshevik culture includes an essay by Daniel Orlovsky on the cultural program of the short-lived Provisional Government. The book concludes with Maurice Reisner's comparative essay on the cultural revolution syndrome in Russia and China.

A reviewer can only congratulate the editors and authors of this volume for a finely textured, multidisciplinary look at a very complex period in Russian and Soviet cultural history. Its overall theme of the interplay between "experimentation and order" is a fresh approach to the subject. It gives new insights into a decade that held out different promises for different groups and produced many unexpected solutions—not all of them premeditated or planned for.

ELIZABETH KRIDL VALKENIER
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NEAR EAST

YLANA N. MILLER. *Government and Society in Rural Palestine, 1920–1948.* (Modern Middle East Series, number 9.) Austin: University of Texas Press. 1985. Pp. xiii, 218. $19.95.

In the past fifteen years the subject of the British mandate in Palestine has progressed from being an area relatively untouched by scholars to one that has produced a substantial number of important monographs. There have been works on British policy (by Michael Cohen and Bernard Wasserstein), Jewish attitudes (by Neil Caplan and Y. Haim), and Arab responses (by Y. Porath and Ann Lesch), as well as specialized studies on immigration (by Moshe Mossek), land policy (by Kenneth Stein), and the effects of the mandate in rural areas, the topic of Ylana N. Miller's recent book.

The author's stated purpose is "to go beyond attributions of responsibility to investigate the concrete conditions that determined and limited Palestinian Arab actions between 1920 and 1948" (p. ix). To pursue that goal she makes two assumptions: first, that village life merits careful study since rural Arabs were the largest element in the population and, second, "that Palestine Arabs were opposed to the Zionist movement from its beginnings in the area" (p. ix). Having said that, she then manages to avoid polemics, one of the pitfalls of this topic, and to produce a study that is thoroughly objective in content.

The body of the book describes the impact that the mandate government had on Arab villagers

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during the period under consideration. Miller views this impact as contradictory: "An administration that entered Palestine with a full commitment to support the development of a Jewish National Home could hardly claim to be perpetuating the status quo. Yet this is what British officials publicly sought in the rural areas under their control" (p. x). Palestinian Arabs were forced to deal with the British government and Jewish immigration simultaneously, a burden far more onerous than that faced by their fellow Arabs in other mandated countries.

Although she describes several aspects of British influence on Arab villagers, Miller seems to regard education as the most important. Since the *Yishuv* (Jewish community) had its own school system, government schools enrolled mostly Arabs. But mass education was never achieved—only one-third of all Muslim children received any education during the mandate period, and even in 1946 only slightly more than half of the villages in the country even had a school. The author attributes these woeful statistics to the government's desire to maintain traditional life in rural areas; education was seen as a threat to this goal. Consequently, most villagers were not able to keep pace with the changes taking place in Palestine, and they were unprepared for the traumatic events of 1948.

Miller's book is based on thorough research in Israeli and British archives, and she uses Hebrew and Arabic sources along with the standard ones in English. Moreover, the work is well organized and conceptually sound. Nevertheless, it bears some of the common trademarks of a dissertation that has been turned into a book. The nature of the topic itself makes it rather colorless, and no personalities emerge from these pages to liven up the narrative. Also, the conclusion fails to tie her thesis together as well as it might, although the introduction expresses her objectives quite clearly. But, despite these minor flaws, this book makes a solid contribution to the field and should take a place alongside the above-mentioned works in the select bibliography of the mandate period.

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Saint Leo College

PHEBE MARR. *The Modern History of Iraq*. Boulder, Colo.: Westview or Longman, Harlow, Essex. 1985. Pp. xvii, 382. $38.50.

Phebe Marr has written a comprehensive historical account of modern Iraq that focuses on the period since the breakup of the Ottoman empire and the beginning of British occupation in 1920. She guides the reader through a maze of unstable politics marked by ethnic and sectarian conflict as well as by intense factionalism based as much on the idiosyncracies of personalities as on ideological divergencies. In Iraq, as in many other Middle Eastern societies caught between traditionalism and modernization, there is plenty of both sectarianism and factionalism. Marr also does not neglect social and economic developments. She has thus produced a fine example of history at its best. In fact, this may well be the best book on modern Iraq now available, especially for those who lack the stamina or the time necessary to digest the voluminous study of Hanna Batatu, *The Old Social Classes and the Revolutionary Movements of Iraq* (1978). Moreover, Marr has written her book in a style easily accessible to students and nonspecialists. The more informed reader will benefit from the wealth of material brought together in one volume.

Iraq has been a troubled country in a troubled part of the world. Located in one of the oldest centers of civilization in the world, the modern state emerged from centuries of neglect and stagnation only to have yet another foreign occupation imposed. The British tried to maintain their position by working with local elites. But the era of British domination already manifested the kinds of problems that were to plague consecutive Iraqi governments. These included ethnic and sectarian fragmentation (the population is divided into Sunni and Shi'i Arab communities, as well as a sizable Kurdish enclave in the north), various strains of nationalism (pan-Arab as opposed to local and regional loyalties), and diverse ideological tendencies ranging from the extremes of communism on the one hand to highly traditional conservativism on the other. Extremes of wealth and poverty also came to characterize the society, particularly with the development of a rationalized cadastral system. As Marr notes, one of the major developments during the first half of the twentieth century was the decline in the nomadic population from 57 percent to 4 percent. This process of sedentarization was accompanied by detribalization and the emergence of gross social inequities. For example, 1 percent of landholders in 1958 owned 55 percent of the land, and many of these were former tribal sheiks. Their tribesmen became tenant farmers. Marr further documents the efforts of a succession of socialist regimes to rectify this situation.

The traditional old regime originally installed under British auspices was finally overthrown in 1958 in a bloody coup. Iraqi politics has not experienced an extended period of stability since (or, for that matter, before). The current ruler, Saddam Husayn, though known as a ruthless and highly authoritarian leader, seemed to offer an opportunity for relative stability and peaceful development when he formally took power in 1979. Unfortunately, in September 1980, he made a disastrous

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error in judgment by choosing to attack revolutionary Iran in the hope of settling an age-old score. As is well known, the resulting war continues; it has already cost thousands of lives, disrupted the oil industry in both countries, and accomplished nothing of major import for either side other than to cement national solidarity.

Marr tells the story of Iraq in a supportive tone. But, unlike some contemporary observers, she does not allow herself to become sycophantic. Each of the leaders is presented in a dispassionate manner, with measured assessments of achievements and failures. Readers who are so inclined can even keep their own score card of victims of each successive regime. If anything is missing from this account, it is a basic explanation of why it has proved so difficult to establish a stable political system in this critical corner of the Middle East. But that may reflect more on Iraq itself than on the author of this excellent book.

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JOHN L. ESPOSITO. *Islam and Politics.* (Contemporary Issues in the Middle East.) Syracuse: Syracuse University Press. 1984. Pp. xviii, 273. Cloth $29.00, paper $12.95.

During the 1970s and 1980s the world has had its attention forcibly drawn to developments in the Muslim community. Whereas in the previous decades of the twentieth century the West had relatively little general interest in the dynamics of the Muslim societies and especially in Muslim thought—many even held the view that Islam would slowly disappear under the impact of modernity—events of the past fifteen years have dramatically thrust the awareness of Islam into public consciousness. It is now widely agreed that we are witnessing an Islamic resurgence.

Particularly striking has been the emergence across the Islamic world, from Morocco to the Philippines, of politically activist movements basing themselves on an ideology derived from Islamic faith. Among their defining characteristics is the insistence that Islam, to be truly expressed and realized, cannot be confined to the sphere of personal piety but must issue into political action. These movements seek to create an Islamic state or states modeled on the teachings of the Qur'an, the tradition of the Prophet Muhammad, and the example of Muhammad's first four "Rightly Guided" successors. Thus, the radicals among them want to change or sweep away existing governments and rulers for the sake of a more righteous dispensation. These radical movements have been responsible for a series of electrifying and disruptive events such as the occupation of the Great Mosque in Makkah in 1979, the assassination of the Egyptian president in 1981, attacks on French and American facilities in Beirut in 1983, and a variety of kidnappings and terrorist incidents.

By far the most consequential and dramatic event, however, was the Iranian revolution, which obliterated a powerful and well-entrenched regime and replaced it with an Islamic government, thus effecting a major realignment of political forces in the Middle East and elsewhere. If the importance of Islam in the modern period had not been clear before, the success of the Iranian revolution has made it indubitably so.

The purpose of John L. Esposito's book is to set the background and context for the contemporary Islamic resurgence as well as to describe how Islamic factors are affecting present-day Muslim societies. The treatment begins with a brief survey of Muslim political thought in the early period of the community's history, goes on quickly to consider medieval positions, and then concentrates its major attention on more recent developments. A chapter on revival and reform offers opportunity to study premodern revivalist and jihād movements and to consider the nature and dynamics of Islamic modernism. The author then turns to the rise of nationalism, perhaps the single most important of all forces that have shaped Muslim thinking in the present century. He traces the rise and impact of nationalism among the Turks, Arabs, Iranians, and Indian Muslims (which culminated with the creation of Pakistan). Two further chapters complete the volume. That on contemporary politics analyzes the role Islam has played in public life in Libya, Pakistan, Iran, and Egypt in recent decades. The concluding chapter on issues and prospects sketches the framework within which Islamic political thought is likely to develop by identifying the chief problems that it faces, problems such as the nature of an Islamic state, the adaptability of Islam to change, and the role of women.

Despite the complexity of its subject, this volume is notable for its clarity and readability and for the felicity of its organization. Though succinct, the treatment misses none of the essential issues and developments; it offers a rounded, insightful, and authoritative introduction to a relatively unknown, though vitally significant, field of inquiry. Among the flood of books that has appeared on Islam in the last few years it stands out for its breadth; its great merit is to offer an informed summary of Islamic resurgence, one that provides the orientation and much of the information needed to understand that phenomenon. The book is thus an invaluable contribution to the literature on modern Islam, useful

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alike for officials, academics, general readers, and students.

CHARLES J. ADAMS
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AFRICA

JAN VANSINA. *Oral Tradition as History*. Madison: University of Wisconsin Press. 1985. Pp. xvi, 258. Cloth $22.50, paper $9.75.

Jan Vansina's new summing up of the methodology of oral tradition is a timely advance on his *De la tradition orale*, published a quarter-century ago. Since then, a cohort of younger scholars has gone to the field and returned, written dissertations based on oral tradition, and contributed new information on the utility of oral sources that needs to be integrated into our methodological understanding. The heyday of such research was in the second half of the 1960s and the first two-thirds of the 1970s. A rather high percentage of more recent postgraduates seem either too unimaginative or too burdened by their concern with the present to pay attention to developments that took place before the last quarter of the nineteenth century.

Oral Tradition as History is not likely by itself to trigger off a new wave of interest in areas of Africa where written documentation is of shallow reach into the past. That will depend on how well the younger scholars, now professional academics, can pass on the excitement of their work to their own students. Vansina can and does provide a relatively straightforward description, in logical progression, of how people generate accounts ("messages") of what they or others have seen or experienced; how people have selected and reformulated materials from these "messages" into bodies of information, formal and informal, which can be passed on to others; and how social context, recognized genres, and performance values in the retelling of such information further mold the content and structure of what is remembered. He then leads the reader through the myriad interpretive paths the historian must tread to turn oral traditions into useful evidence. The metaphors and stereotypes of oral literature and daily discourse in the society, the general world view, and the social and ideological purposes served by the possession and telling of information about the past require that the student of oral sources be sociological observer as well as historian. The social variability of humanity means that what Vansina gives are guidelines, partially illuminated by particular examples. The flexibility and imaginativeness with which they are to be applied must be learned inductively, through reading a wide selection of the best theses and books of African history that make extensive use of oral tradition.

My disagreements with Vansina are relatively small ones. For instance, I agree most strongly with his observation that we ought not to assume without good reason that the explicit meaning of a tradition is not also its implicit meaning. But his example could have been better chosen. He thinks it best to consider Kinguli a single historical figure as tradition does. But Joseph C. Miller's view (in *Kings and Kinsmen* [1976]) that Kinguli was a title held by several successive rulers is supported by usages elsewhere in Angolan history and by the parallel case of the Luvale "chief" Chinyama, apparently part of the same diaspora from southern Zaire as Kinguli, for which there is strong supporting evidence in the traditions themselves for the interpretation of the name as a title with a number of successive holders. On dating, Vansina's pessimism about using droughts and famines to establish chronologies does not seem to take account of the work of Kennell A. Jackson or Robert Cummings on the Kamba of Kenya. It might also be pointed out that the tone Vansina takes on genealogical dating is considerably more deprecating than the content of his discussion would seem to warrant.

My complaints against the publishers of the volume are rather more substantial. Vansina does not deserve the shoddy copyediting (or perhaps the lack of copyediting) that his book received. Hypercorrections, such as the use of "as" when "like" was the proper word, failed to be caught (p. 106). Other errors were probably cases where editors thought they knew better than the author ("A.D." for "A.H." on page 30). A style of using as little punctuation as possible seems to have been followed, frequently to the detriment of intelligibility. Although I noted no obvious cases of deletions of whole lines, I did note a number of examples where several words were missing from a sentence; for example, "Even villages created by Kuba kings to house opponents or Kuba clan sections punished by kings to take up residence in a servile village remember and tell" (p. 107).

In short, *Oral Tradition as History* is an essential addition to the basic literature of African history. But Vansina deserved far better from his publishers.

CHRISTOPHER EHRET
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J. F. ADE AJAYI *et al.*, editors. *Historical Atlas of Africa*. New York: Cambridge University Press. 1985.

The *Historical Atlas of Africa* is a large (twelve by sixteen inch) volume comprising seventy-two one-

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page essays, contributed by fifty-seven Africanists, that together cover the history of the African continent from the origins of humankind to the present. The texts are enhanced by well-chosen photographs, engravings, sketches, and paintings. Paired with each essay is a facing page of specially commissioned multicolored maps, more than three hundred in all. The volume constitutes a worthy introduction to the multidisciplinary approaches developed by scholars in African studies over the past quarter-century. Appropriately, the first "history" section, on human origins, is preceded by contributions on relief, rainfall, vegetation, and soils (by Akin Mabogunje and Paul Richards); geology and natural resources (Kingsley Ologe); language classification (Paul Newman); tsetse fly, trypanosomiasis, and livestock (George Murdoch, Desmond Hill, and Richards); and past climates and vegetation changes (Thurston Shaw). Subsequent contributions are both area-specific and topical, for example, "Roman North Africa" (J. A. Ilevbare), "Kharijites, Fatimids, and Almoravids" (Paulo Fernando de Moraes Farias), "Central Africa, 1600–1800" (Jan Vansina), and "Europe and the African Economy c. 1900" (R. J. Gavin and Richards).

Despite their brevity, the essays are generally of high quality. Inevitably, specialists will disagree not only with the choice of contributors, their selection of themes, and some assertions regarding controversial and unresolved issues but also with such matters as vocabulary usage, for example, "fetish shrine" (p. 37). Significant differences of opinion exist concerning demarcations on maps of African states and societies, and one would have expected more crosslisting of alternative spellings in the index; for example, Gabu is also rendered Kaabu and Ngabou.

Scholars teaching African courses will welcome the *Atlas* as a valuable reference work and for class assignments, especially for readings introducing sections of survey courses using texts deficient in maps. Non-Africanists will want to peruse this attractive and informative volume—Americanists to consult the essays on the slave trade and the African diaspora; Middle East specialists for the sections on North Africa, the Horn, and East Africa; and European scholars for such topics as imperialism, colonial rule, and missionary endeavors. Predictably, all will browse other sections. J. F. Ade Ajayi and Michael Crowder deserve high praise for assembling such a capable group of scholars, cartographers, and bookmakers and for raising publication funds from private sources. The *Historical Atlas of Africa* is unlikely to be superseded soon.

GEORGE E. BROOKS
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DAVID BIRMINGHAM and PHYLLIS M. MARTIN, editors. *History of Central Africa*. In two volumes. New York: Longman. 1983. Pp. xi, 315; 432.

In the curious geographical terminology employed by Africanist historians, Central Africa has always been a residual category, occupying most of what is left after West, East, North, and South Africa have been subtracted. It is perhaps because of this ambiguous status that Central Africa, covering an area larger than India, has never been the subject of a multivolume, multiauthor survey of the type that has existed for most of the rest of Africa for over a decade. This pioneering work edited by David Birmingham and Phyllis M. Martin not only fills an enormous gap in the literature but also introduces new themes and interpretations that may well set the research agenda for the foreseeable future.

Volume 1 is path-breaking in both of the above senses. Birmingham opens the book with a sweeping survey that traces the early development of the farming communities that structured political and economic competition. Dennis D. Cordell's survey of the northern savanna is far more comprehensive than anything previously written on this area. Jan Vansina's magnificient chapter on the peoples of the equatorial forest draws on an astonishing variety of sources to create the first synthesis of life and history in a vast region that has been largely left out of the history books. He corrects stereotyped notions about the role of migration in African history, and he shows that forest societies were structured by competition among "big men" and did not necessarily conform to the familiar lineage models. The chapters by Joseph C. Miller, Thomas Q. Reefe, Alan K. Smith, and D. N. Beach cover more familiar ground, but they do so in ways that differ markedly from previous efforts.

All of the authors in the first volume show remarkable agreement on the major trends and processes in Central Africa. In the first place, the authors emphasize competition at the local level. They see social structures as frameworks within which competition for resources and prestige took place. This local competition is seen as the engine of change in early Central African societies. Accordingly, the authors look at social differentiation between rich and poor, weak and powerful, in all its forms. Slavery in African societies gets due attention as an indicator of social inequality. Gone is the heavy emphasis on state formation as the central process of precolonial history. Even in writing about the eastern savanna, home to some of the largest empires in precolonial Africa, Reefe notes that "the development of large-scale states was the exception, not the rule" (p. 175). In the second place, the authors have a sophisticated view of the role of long-distance trade. Whereas trade was once viewed

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as the generator of economic and social change, it is here viewed in the context of preexisting local conflicts. As Miller cogently sums up the impact of the world economy, "People of every category turned new opportunities flowing from the Atlantic zone to old ends" (p. 119).

The second volume, which covers the colonial period, also breaks new ground. The authors concentrate on the gradual incorporation of Central Africa into the world economy. Martin opens the volume by noting that "the formal imposition of colonial rule was a less decisive watershed for central Africans than once appeared to be the case" (p. 4). She emphasizes the violence of early colonialism and points out that the very feebleness of the intrusive forces encouraged arbitrary and deadly actions. As colonialism became established, capricious violence was succeeded by the "structural violence" of forced cultivation, labor migration, and ecological degradation.

This "structural violence" and its relationship to the international capitalist economy are the dominant themes of volume 2. "Anger and sadness are the constant companions of the student who explores the history of the people of Mozambique, Malawi and Zambia over the past century," warns Leroy Vail (p. 200). Thus, Ralph A. Austen and Rita Headrick trace demographic decline and the impact of rubber concession companies in equatorial Africa; Jean-Luc Vellut, Vail, and Ian R. Phimister trace the impact of mining interests on African life; Bogumil Jewsiewicki chronicles the decline of rural life in Zaire; and W. G. Clarence-Smith traces the impact of plantations in Angola. Together they tell a sordid tale of foreign oppression and African suffering that spans French, German, Belgian, British, Portuguese, and Spanish colonialism. Any illusions about colonialism as a benign phenomenon will be dispelled in reading this book.

What the authors do best is to outline the links between European investors, colonial entrepreneurs, and colonial administrators who together created the conditions with which Africans had to cope. They show that capitalism was nowhere a monolithic or even a coherent system. Interests of state and capital often conflicted, and different groups of European entrepreneurs sometimes fought bitterly over control of resources and labor.

Limitations of space, however, prevent most authors from dealing adequately with African responses to the new conditions. Jewsiewicki exaggerates when he writes that colonialism "deprived African societies of choice" (p. 95). It restricted the choices to be sure, but, within the framework of colonial life and capitalist activity, Africans exhibited enormous creativity in restructuring their lives and societies to make the best of bad conditions and in resisting colonial impositions in a variety of ways. The diversity of African responses to the colonial situation is acknowledged but not adequately developed. These gaps in the analysis implicitly set an agenda for future research. One of the most stimulating features of the second volume is the wealth of suggestive comments that beg further exploration.

The boldest part of the *History of Central Africa* comes in the last two chapters, which cover the period from roughly 1960 to 1980. It has been almost standard practice to end surveys with the demise of colonialism and, thus, to avoid the more difficult issue of why independence did not fulfill African nationalist dreams. Crawford Young, writing about the states that gained independence about 1960, handles the problems with great sophistication and finesse. He shows structural continuities between the colonial and postcolonial periods, but he also shows that independence gave the new African elites considerable room to maneuver, either for good or for ill.

Much of the southern tier of Central Africa achieved independence only after wars of liberation that lasted into the 1970s. These wars were complex affairs, fought with internal rivalries and great power interventions. Birmingham and Terence Ranger skillfully guide us through these events in a chapter exemplary for its clarity. They interweave the theme of political liberation with that of social transformation resulting from growing tensions between town and country. Unlike the northern states where urban unrest led to independence, it was rural discontent that fueled the southern liberation movements. The chapter closes with political liberation achieved but with tensions between town and country posing continued problems for the new rulers.

This is a magnificient work. We have waited a long time for a survey of Central African history. Our patience has been well rewarded.

ROBERT HARMS
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STEPHEN ELLIS. *The Rising of the Red Shawls: A Revolt in Madagascar, 1895–1899.* (African Studies Series, number 43.) New York: Cambridge University Press. 1985. Pp. x, 214. $44.50.

The study of African resistance movements against the imposition of colonial rule has remained a vital field of study for two decades. Stephen Ellis's book, which can be hailed as an important extension of that tradition, traces back to the efforts of T. O. Ranger and others at the University of Dar es Salaam in the mid-1960s. Although the so-called Dar es Salaam school is now scattered, the influence of those seminal years can be seen reflected in many of Ellis's concerns.

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The Rising of the Red Shawls refers to a popular insurrection against a recently imposed French colonial regime and its African collaborators on the island of Madagascar in 1895–96. More specifically, the author uses the vantage point of crisis to examine in detail the workings of the Merina kingdom—its regional, ethnic, class, and confessional divisions as they influenced the motivation, organization, and course of the rebellion. After a chapter tracing the decline of the Merina kingdom during the nineteenth century and a second on the provinces of the kingdom, a detailed analysis of the successive stages of foreign conquest and spontaneous and uncoordinated uprisings in the various quarters of the kingdom and island is presented with clarity and balance. This is the core of the study, making important contributions to our understanding of the motives and methods of this highly unusual rebellion.

I say unusual for two reasons. The author convincingly portrays the motivations of both Merina peasants and nobles as being highly patriotic, reflecting a deep commitment to ideas of homeland and historical identity that I found to be exceptional among nineteenth-century African peoples. The relatively minor role played by millennial expectations, despite a long and close exposure to Christian ideas and an intimate association between an established church and the Merina state before the colonial conquest, was also unusual if far from unique. The role of economic grievances in motivating the uprising and of petty traders in organizing and leading the rebellion in its early stages is also well presented if somewhat less surprising.

Less well presented and convincing is the effort to analyze the motives of the various members of the Merina ruling class in their decisions to join the uprising or to collaborate with the French in its suppression. Although the Merina elite remains somewhat shadowy, the author is able to uncover a series of intrigues and deceptions involving the elite but perpetrated by the French administration. In trying to justify his harsh suppression of the rising and the judicial murder of a high Merina political figure for reasons of state, Joseph Gallieni, the governor and military "hero," engaged in a number of unscrupulous acts, including efforts to falsify the historical record by forgery. The author's handling of this controversial issue stands out as a high point of the book.

One or two shortcomings of this generally excellent work must be noted. The strength of the book is analysis, its weakness narrative. Despite a dramatic beginning involving a description of the murderous attack by the *menalamba* rebels on two missionaries and their child, the drama of events tends to be submerged beneath a welter of detail. This is compounded by the fact, unavoidable to be sure, that Malagasy personal and place names are extremely long and strange to the English ear (for example, Andrianampoinimerina, the first Merina king). More serious is that the three maps included by the author prove of scant use in tracing the events of the narrative and provide little sense of the geographical features and no relief from the maze of mystifying place and ethnic names and seemingly arbitrary boundaries. Ellis does provide a concise and admirable description of Merina social structure, but his exposition of Merina history before 1895 is weak. A clear and cogent description of Merina religion and state ideology is offered, but only a vague and sometimes muddy picture of relations between the Merina and European nations in the preconquest era emerges. Finally, although use is made of concepts such as rebellion and banditry in describing Merina events and movements, no mention is made of some important literature on those subjects; for example, E. J. Hobsbawm's name is nowhere mentioned. Ellis makes no attempt to compare the Merina experience of conquest, rebellion, banditry, Christian conversion, or defeat to that of other colonial peoples. Both a discussion of the wider literature and occasional comparisons would have improved Ellis's otherwise careful and intelligent analysis.

The book itself is extremely well produced as is customary with the Cambridge African Studies Series. The price is also customarily and outrageously high.

EDWARD I. STEINHART
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NIGEL WORDEN. *Slavery in Dutch South Africa.* (African Studies Series, number 44.) New York: Cambridge University Press. 1985. Pp. xiii, 207. $42.50.

Nigel Worden is one of a handful of scholars currently studying the slave system of the Cape of Good Hope under Dutch rule (1652–1795). Like his fellow workers, Worden employs quantitative methods and insights drawn from the study of plantation slavery in the Americas. His book is the most comprehensive analysis of Cape slavery yet to appear.

Worden's book will interest historians concerned to trace the origins of South Africa's modern racial order. Worden drives one more nail into the coffin of the hoary theory that this order originated on the colony's distant pastoral frontiers. Instead, he says that the most rigid pattern of white domination over blacks emerged in the arable southwestern Cape, where slavery was most deeply entrenched. The widespread distribution of slaves among the white population enhanced the assumption that whiteness conferred automatic authority over blacks; it simul-

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taneously inhibited the development of sharp class differentiation among whites. Similarly, little differentiation emerged among slaves; few slaves were put in charge of other slaves at the Cape, and ethnic and status distinctions among them were constantly eroded by the rapidity with which they were sold from farm to farm as a result of the Dutch system of partible inheritance. Moreover, the boundary between the slaves and owners was rarely crossed because the rate of manumission was extremely low.

Worden's book should also receive a warm welcome from students of comparative slavery. Fully cognizant of the literature on American and Indonesian slavery, Worden is explicit about what made the Cape unique. The colony lacked a major staple export that could be the basis for a plantation system. Slaves were used mostly in wheat fields and vineyards, and the closest parallels to the Cape system are found not in the contemporary Americas but in ancient Italy. Moreover, the Cape slave system had not emerged locally, as it had in the North American colonies, but was imported intentionally, and with a complete system of enforcement, by the Dutch East India Company from its Asian possessions. Not surprisingly, then, much of Worden's analysis deals with the role of company government in shaping Cape society.

In arguing that Cape slavery was unique in important ways, Worden does not lapse into the mythology, long favored by more romantic South African scholars, that Cape slavery was unusually benign. With a wealth of detail he illustrates the brutality of punishments at the Cape, the bad working conditions, and the paucity of opportunities for slaves to gain freedom or, having gained it, to advance in Cape society. Moreover, he explains these phenomena in terms of the economic, geographic, and demographic conditions of the colony.

Worden is a cautious scholar, and it is no criticism to say that few of his conclusions are startlingly new. Whenever possible, he seeks to integrate his own extensive data with suggestions made by other workers in the field. As a result, this work is extraordinarily authoritative for a young scholar's first book. Although it leaves many lines of investigation open to be pursued by other scholars, its analysis will not soon be superseded. It is a model of lucid, measured, and meticulous scholarship.

RICHARD ELPHICK
Wesleyan University

JON LEWIS. *Industrialisation and Trade Union Organisation in South Africa, 1924–55: The Rise and Fall of the South African Trades and Labour Council.* (African Studies Series, number 42.) New York: Cambridge University Press. 1984. Pp. x, 246. $69.50.

Jon Lewis's concern in this book is white union activism in South Africa during the first half of the twentieth century. Linking changes in the organization of production to the forms and strategies of trade union organization, he focuses on several influences on the patterns of labor organization: employers' efforts to organize their labor supply and reduce the wage bill through racial discrimination, pressures from white workers to safeguard their jobs, and the push for higher productivity. The author's major argument is that those processes combined to undermine trade union solidarity, stymie limited initiatives toward multiracial labor organizations, and maintain racial hierarchy in the division of labor.

Lewis believes that craft unions' control over skills led to a general conservatism that initially enabled these unions not only to protect jobs without the color bar but also to ally with more militant industrial unions and to support at least some of the Trades and Labour Council's nonracial policies. The industrial unions, however, sought to strengthen their influence by extending their base toward nonracial unionism. Over time, the transformation of the labor process eroded the control of the craft unions, which then moved toward racial exclusion to protect their status: job categories became a matter of race rather than of skill. Racial exclusion, however, required state power. The craft unions therefore became increasingly dependent on the state and accessible to its ideology of cultural nationalism and racial purity.

As the craft unions moved toward the state, the industrial unions experienced a different sort of disorganization. Attacked by capital and the state, the industrial unions sought both to retain the support of white workers, who were moving toward racially discriminatory rules, and to avoid competition with African labor. Possessing an extraordinarily malleable ideology and little solid antiracist consciousness, white initiatives toward a nonracial workers' alliance were repressed, dismissed, or ignored.

Rejecting the position that Afrikaner nationalism and racism explain the trajectory of white union politics, Lewis points out that racial segregation was a constitutent element of, not a barrier to, capitalist development in South Africa. Similarly, the introduction of the color bar was neither an irrational nor simply a political concession to white workers; rather, it facilitated restructuring the labor process. The declining power of the older, English-speaking craft unions was a far more serious challenge to trade union activism than were the efforts of Afrikaner nationalist unions to dominate the labor movement.

Lewis documents carefully the link between the reorganization of the labor process and the trans-

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formation of trade unionism. As skilled workers became more firmly established as technicians and supervisors, the goals and patterns of their unions changed. The urban migration of relatively unskilled whites pressed the state toward a more active role in creating employment, and the increasingly interventionist state allied with capital to dilute jobs. Mechanization and deskilling undermined the craft unions, increased the importance of racial exclusion, and entrenched the government as the working partner of industrial capital.

Lewis explores concrete manifestations of the complex entanglements of race and class: just as in an earlier era mineowners had used race to generate and control a labor force, so white workers sought to use race to protect their situation. Racism, of course, had a dynamic of its own and was never simply a derivative of class conflict. The racism here, however, was not a general ideology but a specific construction in a concrete setting.

The specificity of Lewis's study is both a strength and a weakness. The analysis of white unionism in South African industry is rich and detailed. At the same time, the major problem, which Lewis himself acknowledges, is the inattention to African workers. Although Lewis is quite clear that his study is of white unions, the logic of his analysis does not easily tolerate that disjunction. To maintain nonracial unions in a context of eroding white workers' support and increasing government harassment cries out for the complementary discussion of the ideology, attitudes, and tactics of African unions and the impact of the transformation of the labor process on them. A second problem, also stemming from the specificity of Lewis's focus, is his limited attention to mining and agriculture. Because of the competition for labor, we need to know more about the labor process, the interest and strategies of capital, and the laborers and their unions in these other sectors.

Addressing both the collapse of nonracial unionism and the interconnections of race and class in the context of changes in the labor process, Lewis's work is an important corrective to the labor and social history of South Africa.

JOEL SAMOFF
Stanford University

PHILIP H. FRANKEL. *Pretoria's Praetorians: Civil-Military Relations in South Africa.* New York: Cambridge University Press. 1984. Pp. xxii, 215. $44.50.

Despite its fadish title, Philip H. Frankel's analysis of the role of the South African military provides a well-argued, theoretically informed comparative account of the increasing militarization of this unhappy society. Although short on inside information and original research, the book sheds light on the largely unexplored interests, ideologies, and institutional locations of the crucial pillars of the minority regime. With the war being moved from the bush in Namibia to the streets of Soweto, the real defenders of apartheid are in the spotlight.

Frankel's focus on relationships between civilians and the military in political decision making leads him, together with many other observers, to speculate that escalating violence "could well trigger military intervention, not only in the limited sense of strategic action to uphold the existing government, but in the more fundamental sense of displacing civilian authority as a demonstrably incompetent mechanism for upholding the state and the interests of the military within it" (p. 171).

This popular thesis of a suspension of white democracy in favor of a reformist military repression of black and white extremism does not apply to South Africa for three reasons. First, the close working relationship between the military and the ruling National party, which consolidated with the ascendancy of former defense minister P. W. Botha, has made the military an integral part of government decision making. The military therefore has no need to attempt a formal takeover. Unlike the situation in many black African or Latin American states that have a history of military takeovers, the South African army leadership is on the same ideological wavelength as the government. Support for the extreme Right as well as for the liberal opposition is negligible among South African colonels, in contrast to the right-wing sentiments among the lower echelons of the police.

Second, unlike the army in some Latin American states, the South African military does not constitute a separate caste: more than 90 percent of its personnel are conscripts and often reluctant ones. These draftees reflect the divisions in white society. Their morale, already called into question by the use of the army in a civil war, would be further undermined if they were called on to serve an obvious military dictatorship. The fewer than ten thousand professional soldiers could not control this advanced industrial society of 30 million if confronted with internal resistance. A government perceived as illegitimate by the black majority needs at least minimal support from its own minority constituency if it is to survive.

Third, the South African military still upholds the British tradition of loyally serving the civilian government in power. That does not preclude senior officers from becoming politicized and taking an active part in intergovernmental rivalries. It is well known that high-ranking members of the South African Defence Force frequently counteracted the Department of Foreign Affair's policy calling for more peaceful neocolonial relationships with neighboring states by actively working to destabilize Marxist-oriented governments. South Africa's polit-

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ical isolation has cut off its military from foreign training and posting opportunities. That, in turn, had led many senior personnel to adopt parochial and regional outlooks at the expense of realistic, global assessments. In a crunch, however, dissenting generals would rather resign or be fired than lead a putsch.

By highlighting the shifts in "civil-military boundaries," Frankel has made a crucial, though not always accurate, contribution to the question "Quo Vadis South Africa?"

HERIBERT ADAM
Simon Fraser University

ASIA AND THE EAST

HOK-LAM CHAN. *Legitimation in Imperial China: Discussions under the Jurchen-Chin Dynasty, 1115–1234.* (Publications on Asia of the Henry M. Jackson School of International Studies, University of Washington, number 38.) Seattle: University of Washington Press. 1984. Pp. xii, 167. $35.00.

Hok-lam Chan here intermingles interests in political legitimation and in the Chin state established in the twelfth century by Jurchen tribes of Manchuria, which dominated North China before the Mongols swept over all East Asia. His introduction usefully reviews general theories of legitimacy developed by modern Western sociologists and historians and, against that background, traditional Chinese ways of considering political legitimacy. This is followed by detailed descriptions, analyses, and interpretations of two rounds of discussion about dynastic legitimacy at the Chin court between 1194 and 1214 and a tracing of considerations of legitimacy in China thereafter, even into the People's Republic after Mao's death. The final part of the book offers fully annotated translations of the slim extant Chinese documentation of the Chin discussions.

The question of Chin's legitimacy was hardly at issue. The challenge was to determine how Chin fitted or should fit into one subcurrent of traditional Chinese thought on legitimacy, the theory of the "five agents" (*wu hsing*). This posits a pulsating cycle of cosmic forces perpetually overpowering one another (or generating one another) in dominance over the universe. The five agents are commonly identified in sequence as earth, wood, metal, fire, and water. Associating Chin with one such force would, most importantly, link it ideologically with T'ang, the prestigious prior Chinese dynasty; claim rightful inheritance of the Mandate of Heaven from Northern Sung; or in other ways situate Chin advantageously vis-à-vis its rival Southern Sung. Or it might achieve more than one of these goals at once—all in the hope of strengthening the hand of an uneasy current ruler, shoring up the morale and confidence of the military, keeping Chin's Chinese subjects mollified, and demoralizing external enemies. These discussions were the last serious outburst of that particular Chinese brand of scholasticism, which soon gave way to the Neo-Confucian view that legitimacy derived from a dynasty's moral uprightness and de facto political unification.

In 1977, Michael C. Rogers summarized the Chin court debates (*Sung Studies*, no. 13: 57–66). Rogers noted that Chan's work was already in press, and Chan refers to the stamina required of him "to transform an obsessed idea into published form through several laborious years" (p. xii). It is to Chan's credit that he seems to have squeezed the last iota of conceivable relevance out of his sources. But I am puzzled by several challengeable assertions, one of the least noteworthy and most traditionalistic being that Confucius edited or reconstructed China's five ancient classics (p. 25). I also regret that Chan's editors did not rescue him from avoidable repetitions, ungrammatical English, and wooden and sometimes questionable translations. Most of all, I regret that Chan has not channeled his commendable zeal into some study of broader appeal and greater importance.

CHARLES O. HUCKER, EMERITUS
University of Michigan

DAE-YEOL KU. *Korea under Colonialism: The March First Movement and Anglo-Japanese Relations.* Seoul: Seoul Computer, for the Royal Asiastic Society, Korea Branch, Seoul. 1985. Pp. xvii, 340. $20.00.

It is important to note the subtitle of this study. This is not a long-range study of Korea under colonialism, for which we must continue to rely on Chong-sik Lee's *Politics of Korean Nationalism* (1963) and other works. Only chapter 1, entitled "Korea before 1919," deals with the long-range themes implied by the main title. Therein, Dae-Yeol Ku shows that "gradualism which had been the gist of Itō Hirobumi's civilian regime before annexation" was "swept away" and a very harsh style of Japanese rule installed under the "new criterion" of "simple yet effective" administration (p. 11).

This sets the stage for chapters 2 through 6, which explain in great detail Korea's March First (1919) movement for independence, the surrounding events, and the immediate aftermath. Thus, chapter 2 stresses the origins of and preparations for the uprising; chapter 3, the early demonstrations and their effect on Koreans in exile. Chapter 4 takes up the Japanese response, including their

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brutal suppression campaigns and the emergence of Japanese domestic criticism of these excesses.

Chapter 5 is entitled "British Reactions," and consideration of these is a principal theme throughout the rest of the book. Indeed, an important contribution of Ku's study is its analysis of the way in which Japan's suppression of the March First movement and its foreign missionary and journalist sympathizers soured Anglo-Japanese relations until the Anglo-Japanese alliance, in effect since 1902, was doomed.

Chapter 6, "Missionaries in the Uprising," shows that, although only one foreign missionary, the American George S. McCune, actually encouraged the Korean planners of the movement in advance, the Japanese authorities, assuming the complicity of all missionaries, arrested many of them and antagonized most. The beating of an English missionary, John Thomas, by Japanese police and its aftermath are reported in detail. There follow two chapters on Admiral Makoto Saitō and his efforts to calm the situation in Korea and on Japan's disturbed relations with the British after he became Japan's governor-general at Seoul in September 1919.

Chapter 9 discusses the Chientao incident of October 1920, when Japanese troops crossed the Yalu River to punish Koreans who were carrying on anti-Japanese activities in Manchuria. British and Canadian missionaries and consular officials in Manchuria reported wholesale burning of villages and executions of Korean males, many of whom were Christians. Chapter 10, entitled simply "Conclusions," draws together the various threads of the story, emphasizing Korean restiveness under Japanese rule as the primary cause of the March First movement, but with international repercussions emanating from what was seen by foreigners as a brutal Japanese crackdown. The Japanese reaction elicited British admonitions, which convinced Japan to send Admiral Saitō to Seoul to improve the situation, but the Chientao affair negated that, and British-Japanese relations did not improve.

Ku's conclusions are somewhat contradictory, both as to the nature of the movement, which combined domestic and international causes, peaceful yet sometimes violent, and to the Japanese relationship with the British, who accepted Japanese suzerainty over Korea yet remained unhappy with it.

Actually, Ku assumes that his readers have general knowledge of East Asian international history and can make their own conclusions from the evidence, which he presents in excellent depth but rather episodically. This is primarily a book for specialists, and it contains an excellent bibliography of source materials and secondary works in English, Korean, and Japanese, together with an appendix giving brief biographies of British, Japanese, and Korean individuals important to the story.

HILARY CONROY
University of Pennsylvania

JUDITH M. BROWN. *Modern India: The Origins of an Asian Democracy*. (Short Oxford History of the Modern World.) New York: Oxford University Press. 1985. Pp. xvi, 429. Cloth $29.95, paper $12.95.

Judith M. Brown has given us a masterful, in-depth study of Indian political history under British rule, with an epilogue on India from 1947 to the late 1970s. Her main goal is to investigate "the origins of India's independence and democratic political system" in the context of "the structure and economic base of society" (p. 1). Brown makes excellent use of the many regional and local studies published during the last three decades, integrating their findings into her narrative and analysis and citing them in extensive footnotes and a comprehensive catalogue of suggestions for further reading.

Brown only occasionally mentions ideas and ideals, although those of M. K. Gandhi are especially well explained. Her emphasis is on politics at local, provincial, and subcontinental levels and on the links between them, because "this hinge-making, brokerage, activity between people with varied concerns determines the larger patterns of Indian politics" (p. 161). Brown is not always able to demonstrate this hypothesis when all-India events dominate the scene, but she does her chosen work with a will, nailing together the complex structure of the book with apt generalizations. Her deft craftsmanship is also evident in the insertions, at relevent points, of forty-five tables of political, social, economic, and educational data.

This book maintains a level of analysis that may place it beyond the range of many students, although it would complement nicely an introductory text and an anthology of excerpts from the writings of influential thinkers and leaders. Sometimes Brown draws questionable conclusions, as when doubting whether "an age of reform . . . ever existed in Indians' experience" before 1857 ([p. 81]); *pace* Rammohun Roy and his friends) and when arguing that after 1947 "India's rulers had no clean slate on which to draw a new world, and no new instruments with which to create new designs" (p. 347). (Were not a national government free from foreign control, the Constituent Assembly and its 1950 Constitution, and, above all, the enthusiastic support of millions of citizens new instruments for change?)

These and other slips occur but rarely in this latest product of an energetic and erudite mind. If its next edition were to include a fuller account of some crucial events (especially those since 1947), more

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attention to the thoughts and ideals of leading men and women, and an index richer in both names and subjects, *Modern India* could serve for decades to come as a valuable tool in reconstructing the dynamics of India's political history over the past two centuries.

STEPHEN HAY
University of California, Santa Barbara

ANN LAURA STOLER. *Capitalism and Confrontation in Sumatra's Plantation Belt, 1870–1979*. New Haven: Yale University Press. 1985. Pp. xii, 244. $22.50.

This is a disappointing book. The subject is an important one, and Ann Laura Stoler is qualified to undertake it. Unfortunately, she appears to use evidence selectively, makes unsupported statements, and does not treat some relevant matters. In addition, she brings a Marxist bias to her work. Finally, some minor editing would have improved the presentation.

Seeking to write an ethnographic history set in Indonesia, Stoler aims to describe the "relations of power and production that structured the course of plantation agriculture and the lives of those drawn into its field of force." How, she asks, were "social realities and relationships . . . redefined in accordance with, or in direct opposition to, the industry's concerns" (p. viii)? Those "drawn" were Javanese immigrants employed by Dutch and other foreign corporations in Sumatra's *cultuurgebied*, or "plantation belt," popularly known as the "Deli" district. The author's special concern are those Javanese who left estate housing to establish "squatter" communities on the estate periphery in order to reconstitute the villages they left in Java. Stoler examines methods of "labor control," acts of violence against and by workers, worker participation in Indonesia's struggle for national independence, union activities, and changes in workers' values.

History must be separated from anthropology in this book. The anthropology is based on the author's field work in the 1960s and 1970s, especially in the village of Simpang Lama. She found that the residents of the reconstituted villages had duplicated the physical circumstances, ceremonial events, and even the spirit world of Java. But major changes had occurred. Wage employment rather than farming and land had become the focal point of life. The result was a weakening of reciprocal cooperation. The patron-client relationship, a pervasive tradition in much of Java, was largely absent. The Javanese striving for harmony (*rukun*) had been replaced by "dissonance": family members no longer pooled incomes, traditional crafts were weak, the role of children had changed, and the family was no longer cohesive.

The useful insights in Stoler's anthropology are lacking in her history. The history is based heavily and sometimes uncritically on secondary works. But the major flaw is that many matters essential to the author's stated purpose are largely or wholly ignored. Colonial labor policy and administration are unexamined. Little is said about estate employers, whose actions are portrayed as mostly monolithic, despite their frequent division on labor questions in nearby British Malaya, where some of the same Deli corporations operated. No mention is made of small or medium-sized properties. Did they employ workers? They did elsewhere. The size and ethnic makeup of the labor force is not stated for any of the time periods treated. Such data is important because the Javanese were not the only estate workers. To what extent can workers' actions be attributed to non-Javanese? And what of workers' health? Malaria, ankylostomiasis, venereal disease, and malnutrition at times devastated estate labor forces. Debility, death, and the measures taken to deal with them surely helped redefine "social realities and relationships."

Stoler says the idea that a vast Sumatran wasteland was transformed by the pioneering zeal of Western enterprise is a myth. She declares that "ethically impoverished" colonial authorities and capitalists collaborated to exploit workers. "Hegemonic and coercive control" produced "enormously" varied labor resistance. Claiming a "feminist vantage point," she finds that "control over sexuality" and the "manipulation of gender hierarchies" were important means of controlling labor. Estate workers were quiescent during Indonesia's struggle for independence but active in the Communist-oriented labor movement before the Indonesian military's assumption of power in 1965. Exploitation has continued in independent Indonesia with the government, the unions, and, by implication, the World Bank cooperating to this end. Some—perhaps many—of these conclusions are valid, but they would be more acceptable had the author investigated more broadly and taken an even-handed approach.

Stoler has the knowledge, skills, and experience to make a major contribution to the historical scholarship of Indonesia, but this book is not such a contribution. Indeed, it is not good history, ethnographic or otherwise.

J. NORMAN PARMER
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VALERIO VALERI. *Kingship and Sacrifice: Ritual and Society in Ancient Hawaii*. Translated by PAULA WIS-

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SING. Chicago: University of Chicago Press. 1985. Pp. xxviii, 446. Cloth $55.00, paper $22.50.

This book represents an excellent anthropological study of ancient Hawaiian society. At first glance, the work, written by Valerio Valeri, an expert on the original cultures of Indonesia and Hawaii, appears to be a summary of the contributions made by observers ranging from untrained explorers and missionaries to present-day ethnologists and archaeologists. Further reading, however, reveals much new information and studied analysis presented in a systematic fashion. In the introduction the author surveys the literature on the culture of Hawaii in a manner somewhat comparable to a historian's bibliographic essay. But here the works are discussed and evaluated prior to use as sources throughout the succeeding chapters, giving the reader much-needed insight into the author's methodology and evaluation.

In an early chapter summarizing Hawaiian theology, Valeri presents a table listing the attributed characteristics of the four major gods in the Hawaiian pantheon and carefully notes sources for the study of each. He has stated that "different hierarchies, associations, oppositions exist amongst the gods depending on the context" (p. 15). In addition to the natural phenomona listed, each god has a duality that may also be viewed in a human dimension. This is explained with a clarity seldom found in comparable works.

For the author, the institution that both held ancient Hawaiian society together and then made it work was the kingship, which Valeri sees as "the veritable creator of society as a coherent whole" (p. 158). Interestingly, he draws on Charles Wilkes as a contemporary observer who agreed with the thesis.

Although the work has merit, the historian will not necessarily have an easy time using it: several of the concepts are complex, and the use of Hawaiian words to denote them complicates the reading process for the uninitiated. Once past this, however, the reader will find the book an illuminating, useful, and important work in the field.

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UNITED STATES

RUSSELL L. HANSON. *The Democratic Imagination in America: Conversations with Our Past.* Princeton: Princeton University Press. 1985. Pp. xi, 478. Cloth $45.00, paper $9.95.

The Democratic Imagination is indeed an apt title. For, if I understand the exegesis correctly, Russell L. Hanson is less concerned with the history of the democratic tradition in America than with the continuing dialectic or debate illustrated in the rhetoric of changing majorities and minorities, political parties and platforms—or, in Marxist terms, with shifts in the opinions of the ruling class. Hanson disagrees with any attempt at an objectivist definition of democracy, as well as with the so-called Progressive historians' interpretation of democracy as an advance. He sees the contemporary crisis in democracy as the result of a decline in the fervor and quality of this historical debate, and he believes accordingly that it is more important that the debate continues than that any one definition emerges supreme.

The book—though overly ambitious, undisciplined, and formless, as its subtitle suggests—does try to trace democratic theory from the founding fathers to the Progressives. Hanson places heavy reliance on a great variety of secondary works by political scientists and historians—the Progressive school, of course, excepted. Curiously, he hardly considers the way in which wars have affected American democracy.

The first real break with the past occurred during the New Deal, which the author interprets as inaugurating a consumer (that is, a spending) society characterized by equal privileges and material benefits for all. This marked the obsolescence of traditional democracy. Although the Old Left had failed to capitalize on the Great Depression, the New Left also failed in its indifference to women's liberation and in the continuing frustration of American blacks. Nonviolence, it is true, helped whites and blacks cooperate in the civil rights movement, but blacks ultimately perceived that the movement was less a prelude to action than a rhetorical challenge to whites to live up to democratic ideals. Today's prosperity merely confirms the status quo while corrupting liberal democracy by its materialistic goals. The demands of the welfare state accordingly demean the concept of civic virtue and also obstruct the efficiency of administrative (that is, bureaucratic) power. The achievement of the Great Society, the author hints, may depend on both a technocratic revolution and the increasing professionalization of reform.

Obviously, this is a tendentious work. Hanson desires a democracy that is more equalitarian than liberal. The discourse in the dialectic of democracy, which he considers all important, has also, however, to be continually revised to meet this goal. The historian, in his opinion, must be an activist looking to the future as well as to the past. History therefore means both the telling and the making of the story.

ARTHUR A. EKIRCH, JR.
State University of New York, Albany

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THOMAS C. COCHRAN. *Challenges to American Values: Society, Business, and Religion.* New York: Oxford University Press. 1985. Pp. 147. $13.95.

The question of the age is that of the conduct of life; as Emerson asked in 1852, "How shall I live?" How we live depends on what we consider important in life, that is, on our values. American values, and the expressed need to "return" to them, have been much discussed recently, although few have taken the trouble to define these values or to ask whether they are even worth returning to. In this small book Thomas C. Cochran identifies some of our American values, including individualism, private rights, localism and regionalism, material success, resource wastefulness, and impatience with nonutilitarian activities and learning. These values were in place by the early nineteenth century and were appropriate to the dynamic, enterprising, homogeneous society of that time. They encouraged the early development of American democracy and industrial technology. In the end these values helped produce a pluralistic, industrial society that is almost entirely at odds with them; modern Americans are still struggling with that new society and with the cumbersome legacy of those traditional values.

This is the thesis of Cochran's short but wide-ranging book. In little more than one hundred pages, he deals with, among other things, the bureaucratization of business and government, immigration, urbanization, the feminist movement, federal land policy, the impact of Darwinism, the national banking system, progressive education, racism, welfare, and the energy crisis. Even Cochran cannot do justice to such an array of topics, but he does a splendid job of weaving them together and of illustrating how the values that helped make America great now threaten that greatness by keeping us out of touch with our real social environment.

One always welcomes a reflective work of synthesis by an established scholar such as Cochran. If the general thesis is not really new, the book does offer some interesting insights on how America became, in the words of David Mandel, a "centerless, disaffected, alienated, uncommitted and disappointed society." Although the book covers three hundred years of history, its real concern is the 1980s and today's spiritual malaise. For the most part, the book is gracefully written, although occasionally Cochran's copyeditor might have served him better, as in the case of the following topic sentence: "In the long run, a reasonable degree of honesty and of accountability was imposed on railroad corporations by an excess of government regulation, which only increased the lack of incentive in bureaucratic managers" (p. 68). I can recommend this book as a thoughtful comment on "how we got into this" and, more generally, as a reflection on that elusive phantom, the American character.

DONALD H. MEYER
University of Delaware

THOMAS R. COX *et al. This Well-Wooded Land: Americans and Their Forests from Colonial Times to the Present.* Lincoln: University of Nebraska Press. 1985. Pp. xvii, 325.

Students of American history are not likely to overlook the importance of forests in the nation's economic development, but Thomas R. Cox asserts that wooded lands have had a "far more central" role in all of American life "than is generally recognized" (p. xvii). Each of the contributors to this cooperative work reviews the customary periods to show the workings of a "multiplier effect" resulting from forest use and management (p. 20). For the colonial era, the late Joseph J. Malone suggests that wood for fuel and housing, in unlimited and nearby supply, insured healthy living conditions for settlers of all classes; well-being and comparative comfort contributed, in turn, to self-confidence and a resentment of restrictions on access to that supply, for even woodcraft was one of the "many ways of declaring independence" (p. 24). Tracing the rich panorama of the national period, Phillip Drennon Thomas demonstrates that most of American culture was itself a forest product. Materials and technologies for ships and roads and bridges, for mines and the first manufactures, are obvious examples. Consideration of forests by travel writers and novelists are also discussed as "products" in the widest sense of that word. Even the short-lived leather industry and the doleful foundations of federal Indian policy were consequences of the national lust for logging. Thomas's well-told account of the white pine harvest in Maine, its river booming, hardy choppers (saws came later), and mill towns such as Bangor ("a star on the edge of the night," Thoreau called it, "still hewing at the forest from which it is built" [p. 77]) fairly begs for transcription into a television miniseries.

In the next essays Cox and Robert S. Maxwell continue marking the statistics of lumbering and wood consumption for the decades since the mid-nineteenth century, but, just as those quantities increased enormously, so did the complexity of the "multiplier effect." As a result, these two chroniclers must be more selective in the topics they mention. Cox properly realigns the general reader's assumption that the lumber booms were simply a shift from New England to the Pacific Northwest. He builds on the observations of Malone and Thomas about the origins of the impulse for forest conservation to recount the institutionalization of scientific manage-

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ment of forests. He emphasizes that the lumber barons' mendacity and myopia were quite in keeping with the mores of their time. Maxwell—whose chapters covering the twentieth century may be the ones most frequently consulted by students seeking an introduction to forest history—explains that earlier problems were dealt with by relatively simple solutions (tariffs, regulatory agencies, applied sciences, and technologies), but current intensive forest production must be pursued within the context of planet-wide ecological consequences. Yet even the computerized facilities and high profile stewardship of our own time have not allowed us to control the continuing pollution and waste of wooded lands in the United States, much less in Third World countries.

Some statements in this provocative history warrant further discussion. Cox's passing observation that "government itself purchased much lumber for military use" (p. 127) should be traced through the entire chronology, not only in the context of successive frontiers but also in the framework of the military-industrial complex that characterizes the present day and, undoubtedly, the immediate future. Although forest access and management legislation is noted on the federal level, none of the contributors probe the influence of lumbermen and foresters on state and local governments and politics. Indeed, the most revealing and significant example of such influence, the Oregon and California Revested Lands, is not even mentioned. The volume's annotated bibliography will, however, enable researchers to make their own investigations in the many topics of forest history. The publisher uses a small and crowded typeface but should be thanked for bringing out this and several other titles in an increasingly important field of history.

ELMO RICHARDSON
Seattle, Washington

SYDNEY E. AHLSTROM and JONATHAN S. CAREY, editors. *An American Reformation: A Documentary History of Unitarian Christianity*. Middletown, Conn.: Wesleyan University Press. 1985. Pp. xvi, 493. $39.50.

Few anthologies can have been awaited as long as this one. In *The Transcendentalist Ministers* (1959), William R. Hutchison acknowledged that his opening summary of Unitarian theology was "most heavily indebted to Sydney E. Ahlstrom's work in progress, 'The Christian Unitarian Mind: An Anthology of Early American Unitarian Writings'" (p. 4 n). The "work in progress" was shelved while Ahlstrom devoted his attention to his monumental *Religious History of the American People* (1972). But near the end of his life Ahlstrom cooperated with Jonathan S. Carey to bring this massive documentary history of American Unitarian Christianity to light.

The introduction remains as Ahlstrom originally wrote it; like the rest of his work, it is broad in scope, encyclopedic in its grasp of relevant facts, sound and persuasive in its interpretation. This anthology should do for nineteenth-century Unitarians what Perry Miller's great anthology, *The Transcendentalists* (1950), did for their offspring and adversaries. Unlike most of Miller's selections, however, these are usually printed in full. Ample representation is accorded to such Unitarian greats as Ebenezer Gay, Jonathan Mayhew, the Henry Wares, Andrews Norton, and, of course, William Ellery Channing. One selection alone would make this volume worthwhile: Frederic Henry Hedge's Dudleian Lecture for 1851, an uncelebrated masterpiece of American intellectual history.

The decision to include hymns, a catechism, and a liturgical selection in this anthology is welcome, although one wonders why James Russell Lowell's magnificent "Once to Every Man and Nation" and Julia Ward Howe's famous "Battle Hymn of the Republic" are left out. In the printing of Edmund H. Sears's "Calm, on the List'ning Ear of Night," four lines are transposed. And, in their reprinting of Andrews Norton's "The Latest Form of Infidelity," the editors omitted the two appended notes, which enrich and clarify the argument. The absence of Francis Bowen from the anthology is strange, particularly since he could have illustrated the centrality of Scottish moral philosophy to American Unitarianism that Ahlstrom's introduction rightly stresses.

This anthology represents only the portion of American Christian Unitarian thought that was specifically and overtly religious. No feasible anthology could possibly represent the full range of the contribution of Unitarian Christianity to American culture. To do so it would have to include literary criticism and natural science, the classic narrative histories of John Lothrop Motley and William H. Prescott, Richard Henry Dana's *Two Years before the Mast*, the didactic children's books of Horatio Alger, the sardonic wit of Oliver Wendell Holmes the elder, the evocative myth-making of Henry Wadsworth Longfellow's "Hiawatha" and Clement C. Moore's "A Visit from St. Nicholas." By displaying more accessibly than ever before the religious seeds of the fertile cultural flowering of Unitarian New England, this anthology stakes out a major claim on the attention of American intellectual and literary historians.

It is regrettable that the volume did not appear at the time Ahlstrom originally conceived it. Then its juxtaposition with Miller's anthology would have been more readily apparent, and together they would have provided a focal point around which

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much of the secondary literature of the past generation could have orbited. But we should be thankful to Carey for seeing the long-deferred project through to completion at last.

DANIEL W. HOWE
University of California, Los Angeles

MELVIN YAZAWA. *From Colonies to Commonwealth: Familial Ideology and the Beginnings of the American Republic.* (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1985. Pp. ix, 261. $28.50.

Melvin Yazawa has written an ambitious and engaging, though not altogether persuasive, book. His purpose is to examine the dominant patterns of "early American social perceptions" organized around "familial affections." Both "domestic peace" and "harmony in the larger public world" of colonial America, he argues, depended on a common set of cultural dispositions transmitted through childhood education and a wider array of socialization processes. At the center of this "familial ideology" lay the paradigm of patriarchal authority, a calculus of hierarchical but reciprocal relationships that bound society together in "bonds of affection." Family, schools, church, state—all found order in this web of unequal but affectionate parental authority.

In part 1 of the book Yazawa describes the theoretical foundations of affectionate authority. Part 2 offers examples of the ways in which the familial paradigm functioned in everyday life. The school setting, he suggests, illustrates how "inward esteem" was "manifested in outward expressions" of conduct. In part 3 Yazawa examines how the familial paradigm contributed to the movement toward American independence. As king and ministry cast aside parental affection in their dealings with the colonies, their authority weakened and the attacks of radical Whigs gained support among Americans. In parts 3 and 4 the author describes how the revolutionary experience undercut the familial paradigm, eroded notions of patriarchy, and strengthened republican doctrines of social authority, doctrines based not on affection but on depersonalized principles of constitutionalism.

Yazawa's arguments are important to an understanding of the revolution and the nation's founding, and he develops them through a sensitive reading of cultural texts. Yet, ultimately, the book is more suggestive than persuasive. In part, that is because the subject matter is too sprawling and complex to be successfully encompassed in scarcely two hundred pages. But there are other problems as well. Yazawa assumes the correspondence between familial and other systems of social authority instead of presenting it as a theoretical proposition requiring analysis and explication. As a consequence, much of importance is left unexplored. Was the biological bond between parent and child, for example, identical to that between public official and citizen or king and colony, even if the language used to describe all three relationships was in some sense similar? In key sections of the book Yazawa draws exclusively on evidence from New England, yet he claims to be discussing cultural norms characteristic of the middle and southern colonies as well. Moreover, his discussion of colonial America is profoundly ahistorical, for he combines early seventeenth- and mid-eighteenth-century materials as if their social and cultural contexts were the same. Finally, Yazawa uses terms such as "paternal," "fraternal," and "familial" interchangeably when they can take on quite different meanings, and he seems unconcerned that the colonies revolted against their "mother" country, a point, one might think, of some theoretical importance in an analysis of paternal ideology.

JOHN HOWE
University of Minnesota

T. H. BREEN. *Tobacco Culture: The Mentality of the Great Tidewater Planters on the Eve of Revolution.* Princeton: Princeton University Press. 1985. Pp. xvi, 216. $19.95.

Tobacco was synonymous with colonial Virginia. The recent demographic, economic, and social historiography of the colony has hinged on that crop's labor demands and trade cycles. T. H. Breen shows how the role of Virginia's rulers as tobacco planters shaped their culture and politics. As the book's title deftly suggests, the agricultural process of growing tobacco influenced planters' values and behavior and vice versa.

In a brilliant chapter, "Tobacco Mentality," Breen describes how the characteristics of tobacco as a crop affected the use of space and time. The crop had at least a dozen phases of cultivation, harvesting, and processing. Unlike rice and wheat, tobacco allowed virtually no let-up from work. Planters spent most of their time on their plantations, and most of that time was spent attending to the growing of tobacco.

Tobacco growing was central to Virginia's traditional culture. Planters took an active, even compulsive, concern in supervising its production; they were seldom absentee owners. Planters aspired to be "tobacco masters," to whom other growers looked for advice about when to set out seedlings in the spring and when to cut in the fall. The planters' conviction that they exercised control over their crop reinforced their competitiveness and their insistence on autonomy. The measure of their success was the price of tobacco on British markets.

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The planters had the commercial mentality of staple producers and had little understanding of the international marketing of tobacco. Since the 1730s, Scottish factors had bought tobacco directly from small planters; by the 1770s they were buying three-fourths of the colony's entire crop. The big planters continued to ship on consignment and remained dependent on English merchants, who sold their tobacco for them and used the proceeds to provide credit for the purchase of slaves and consumer goods.

The Virginia planters were capitalists. They measured their status by monetary wealth; they were attentive to price; and they bought other people's labor and sold its surplus in order to increase production. But with respect to the crucial exchange relations for increasing wealth in the tobacco market—commercial credit and debt—they had anticommercial attitudes. The planters referred to their relations with English merchants as "friendship." Since the planters granted their business as a favor, they did not consider prices and credit to be determined solely by supply and demand, and they did not view contracts as completely binding. There was a contradiction between their cherished economic autonomy on the plantation and their reliance on the commercial credit that was its prerequisite. To deal with this contradiction, the planters devised a set of rules and manners that allowed debtors to act as though they were independent.

Since Breen chose to present the tobacco mentality as a traditional outlook rather than to analyze its development, much of the discussion has a static quality. In the second half of the book he integrates this wholly original topic with recent fixtures in the discussion of Virginia during the imperial crisis—planter indebtedness, political alienation from Great Britain, and the increase of wheat farming. He argues that commercial alienation and the detection of merchants' conspiracies eventually disposed the planters to appropriate the radical ideology of British opposition politics and to resolve on social reconstruction. This interpretation begs the question of that ideology's irrelevance before the credit crises of the 1760s and 1770s. If it were a part of their culture by the 1750s, then it would have shaped their interpretation of their commercial relations.

In its attention to the correspondence of social values, ecological imperatives, and exchange relations, this book is a model of how a common-sense application of cultural anthropology can bridge economic and social history. This study's strong regional orientation and its placing of everyday economic activity in the context of the world economy make its perspective especially appropriate to the study of early American history.

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Dalhousie University

JEAN R. SODERLUND. *Quakers and Slavery: A Divided Spirit*. Princeton: Princeton University Press. 1985. Pp. xiii, 220. $27.50.

Recent decades have seen an accumulation of creative scholarship on the thought and politics of Quakers in colonial Pennsylvania regarding slavery, including books by Thomas Drake, Richard Bauman, Sydney James, Gary Nash, and David Brion Davis. Jean R. Soderlund cites them all, summarizes early Quaker tracts and statements, but moves quickly beyond them to her own painstakingly careful statistical work, based on minutes and records of Quaker meetings and the tax records and inventoried wills of two Quaker communities in New Jersey (Shrewsbury and Chesterfield) and two in Pennsylvania (Philadelphia and Chester). This study builds on her Temple University thesis, which also drew on the archives of Burlington County, New Jersey, and her work on the *Papers of William Penn*, for which she edited the volume on the beginnings of Pennsylvania. She also uses the Philadelphia Center for Early American Studies and the Swarthmore College Peace Collection, where she is curator.

Her first sets of data show that, even in face of a crescendo of Quaker voices opposing the buying, selling, and owning of slaves, slaveowners included a large proportion of the Quaker leaders in both political and meeting life: no formal step was taken by the regional yearly meeting until 1754, when, after the funerals of prominent slaveowners, it authorized publishing John Woolman's *Considerations* and its own antislavery statement and followed in 1758 with discipline against slave buyers and a visitation committee to counsel all Quaker slaveowners. Yet Soderlund's data show that Quaker meetings recorded few slaves liberated or Quaker owners "disowned" until the 1770s: she ignores unrecorded manumissions, but the story is clear in the wills and in county records of the £200 bonds that New Jersey poor laws required owners to post for each freed slave. (Admittedly, the freeing of slaves after their adolescence or on their masters' deaths was often determined much earlier.)

She breaks down figures comparing Quaker slaveowners with members of other churches and shows that most slaveowners were farmers owning more than 250 acres or town craftsmen and rich merchants. She analyzes data on the jobs, ages, and sexes of slaves and slaveowners and on the proportions of marriages and children at home for slaves and free blacks. She has only a sentence (and a footnote reference to Herbert Aptheker) on violence by and against slaves (a key reason Quakers opposed slavery). But she uses figures that contrast the many slaves freed but the few Quaker owners "disowned" in Shrewsbury, and the reverse in Chesterfield, to support Max Weber's contrast of "exem-

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plary" with "emissary prophecy," and she relies on Jerry Frost's contrast of two kinds of eighteenth-century Quaker reformers: humanitarians concerned for slavery, and purists concerned for Quaker community and family life (she calls them "tribalistic reformers"). Careful as always, she disproves that power struggles within Quaker meetings were the source of campaigns against slaveowners. Her appendixes on the hierarchy of Quaker meeting structures and on the assessment and leadership lists, used as her data bases, might have enriched her introduction.

HUGH BARBOUR
Earlham College

RICHARD AQUILA. *The Iroquois Restoration: Iroquois Diplomacy on the Colonial Frontier, 1701–1754*. Detroit: Wayne State University Press. 1983. Pp. 285. $18.95.

The last decade has produced a multitude of monographs on native American history. Like Richard Aquila's book, many of these works are a result of fellowships from the Newberry Library's D'Arcy McNickle Center for the History of the American Indian. The book considered here is an excellent example of scholarship nurtured by that institution.

Aquila's narrative starts with a survey of the development of Iroquois diplomatic power in the seventeenth century, and then he provides a more thorough examination of the resurgence of Iroquois importance in the first half of the eighteenth century. The work also attempts to relate briefly the results of the Iroquois restoration of power in the latter half of the eighteenth century. This restoration was not haphazard but was "at various times the product of necessity, fortuitous circumstance, common need, chance, and rational geopolitical decisions" (p. 16). To the author's credit, the Iroquois are presented as a people that pursued a course of systematic enlightened self-interest in order to restore their political, military, and economic power by 1754.

No significant work to date has examined the implications of Iroquois diplomacy between 1700 and 1754. This study has value in that it attempts to fill that gap, but, ultimately, Aquila fails to do so. He demonstrates that the Iroquois reasserted themselves militarily, buttressed their society and their confederacy, and enjoyed good relations with the English and the French. Furthermore, Iroquois successes had an enormous impact on other Indians of the eastern woodlands. In the first half of the eighteenth century, tribes such as the Tuscaroras, Eries, and Hurons were brought under the Great Tree of Peace, the Iroquois Confederacy. In a broader context, Iroquois diplomacy, according to Aquila, nurtured colonial economic growth and guided the westward movement while Iroquois "strength determined colonial warfare" (p. 245). Overall, this work gives us a good survey of the diplomacy of the period, but the work is surprisingly superficial. Although the author gives us neither bloodthirsty savages nor noble savages, he does give us faceless and, by implication, vanishing native peoples.

Native American history is the only area of our discipline where lack of exposure to and interest in native peoples today passes for objectivity among such scholars as James Axtell in *The European and the Indian* (1981). Likewise, Aquila has not paid much attention to oral histories and wampum records that seek to present the Iroquois side of the story.

Aquila has given us a good survey of Iroquois diplomacy from 1700 and 1754, but in his work the Iroquois and Indian individuals remain unexamined from a human point of view. Alas, the cigar-store Indian, as a stereotype, lives in such works. Perhaps it will take another generation and new methods using oral traditions before we get a balanced treatment of Indian people. Only when Indian people write and publish their own histories and control more of the means of scholarly production will we begin to obtain a more balanced view. Given the state of the art in native American history, Aquila's work is in the mainstream of current research.

DONALD A. GRINDE, JR.
California Polytechnic State University

PHILANDER D. CHASE, editor. *The Papers of George Washington*. (Revolutionary War Series.) Volume 1, *June–September 1775*. Charlottesville: University Press of Virginia. 1985. Pp. xxvii, 513. $35.00.

The words of George Washington and the recorded responses are fraught with historical significance even after more than two centuries. This handsome volume, which covers only the war period from June to September 1775, is a refresher course for historians and a source of first-hand knowledge for the layman. It marks the beginning of the second important chronological series of *The Papers of George Washington*.

This volume contains papers that for the most part illustrate General Washington's early efforts to mold a fighting force out of the raw and often-unruly patriot troops besieging British-held Boston. In his first official and personal letters, Washington took pains to disclaim any desire to become commander in chief of the new American army. Such statements must be contrasted with the fact that he was the only delegate in uniform at the Continental Congress when it instituted the military forces.

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Washington not only carried on an extensive correspondence with the Congress, other military officers, public officials, and private citizens but also somehow—often at night—took time from his arduous duties to correspond with friends, family, and business associates. True, he had the assistance of capable amanuenses such as Joseph Reed and Alexander Hamilton who could themselves compose proper, if quaintly spelled, communications.

Prominent among the papers about the siege of Boston are general orders concerning the military situation, news of the nearby British soldiers and fleet, acquisition of food and clothing, and discipline among the amateur troops—including one letter expressing virtual horror at the antics of some riflemen caught swimming in the nude near local women. Two letters he wrote to his wife in June 1775 are rare survivals of the correspondence that Martha Washington destroyed just before she died. The value of this volume is greatly enhanced by the superb annotations that explain and adorn many otherwise-obscure references in the documents.

At times Washington even waxed poetic. "I am now," he wrote to a relative, "embarked on a tempestuous ocean from whence perhaps no friendly harbor is to be found" (p. 12). When in despair, he often called on God to help his cause; more prosaically, he called councils of war before beginning any major military operation. Some of his letters were captured by the British, who inserted descriptions of spurious scandals about Washington in hopes that the letters would be published and believed by readers. As an example of the stern discipline meted out, a general order of July 29 states that one James McDaniel, an enlisted man charged with forging an order to obtain a quart of rum, was to receive twenty lashes "to be executed after prayer time tomorrow" (p. 191). Other highlights of this book are the plans for the successful expedition to steal gunpowder from Bermuda, exchanges with British General Gage and Tory leaders, and Washington's impassioned appeal—which now seems ironic—to Benedict Arnold for his success in the ill-fated march to Quebec in late 1775. The book is on the whole a propitious addition to the literature of early American history and to the story of its great leader.

NORTH CALLAHAN
New York University

WILEY SWORD. *President Washington's Indian War: The Struggle for the Old Northwest, 1790–1795*. Norman: University of Oklahoma Press. 1985. Pp. xv, 400. $24.95.

In this book Wiley Sword narrates with authority and style the story of conflicts between Indians and whites in the Northwest Territory from the close of the Revolutionary War to the collapse of Indian power in 1795. Sword has an important subject to deal with, for that time period was filled with more crucial events and more interesting characters than most fictional accounts would encompass.

Unlike many previous authors, Sword assesses the position of the Indians sympathetically and objectively, concluding that after 1783 they were fighting a losing battle against white encroachments on the territory north of the Ohio River. To balance the picture, however, he notes that Kentuckians were not only furious when Indians began raiding their territory but also incensed by the Confederation's "neglect" of Kentucky's defenses. Finally, Kentuckians took things into their own hands by commencing illegal militia raids against Indians both innocent and guilty.

On his accession to office in 1789, President George Washington inherited this deteriorating situation. He did not want an all-out Indian war any more than did his antimilitarist critics in Congress. Hence, he tried to negotiate a settlement. Nevertheless, he was compelled by 1791 to admit that an Indian war was inevitable and, in trying to implement his new policy, had to endure the failure of two army commanders, Josiah Harmar and Arthur St. Clair. At last, Washington employed Anthony Wayne to lead the American army, and Wayne triumphed over the Indians at Fallen Timbers in 1794. A year later, Wayne forced the Treaty of Greeneville on the Indians and extinguished their power north of the Ohio.

In relating these events, Sword interweaves many strands of a complex story and successfully relates all points of view—American, British, and Indian. He also gives a balanced account of the interplay between civil, diplomatic, and military events, thereby producing a sophisticated example of multifaceted military history. He makes good use of printed primary sources and secondary works (except for his omission of Richard H. Kohn's excellent book on Federalist military policy) and grounds his narrative firmly in historical fact. Although he and his editors allowed a distractingly large number of misprinted words to creep into the last third of the text, his book is otherwise attractively laid out, providing the reader with useful illustrations and good maps.

In sum, Sword's description of the Indian wars in the Northwest Territory during the Federalist era is a genuine contribution to American military literature and a useful addition to the reading list of anyone who would claim fully to understand that period of history.

PAUL DAVID NELSON
Berea College

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JOHN SUGDEN. *Tecumseh's Last Stand.* Norman: University of Oklahoma Press. 1985. Pp. xiii, 298. $19.95.

Although most American historians are aware that Tecumseh was killed on October 5, 1813, at the Battle of the Thames near Moraviantown, Ontario, the details surrounding his final days and his death have remained uncertain. Because the British retreat from Amherstburg was disorganized and Tecumseh did not accompany the main body of British troops, reports regarding his activities in September and October 1813 have been vague and contradictory. Moreover, most American accounts of the Shawnee war chief are so embellished with apocrypha that historians have experienced considerable difficulty in ascertaining fact from fiction.

John Sugden's study of the final two months of Tecumseh's life, the Shawnee chief's death, and the subsequent fate of his body does much to dispel the confusion surrounding these events. Based on extensive research in both primary sources and secondary works in Great Britain, Canada, and the United States, Sugden's narrative traces Tecumseh's activities in September and October 1813. He illustrates that Tecumseh did not join the main force of British and Indians who withdrew from Amherstburg on September 27, 1813, but that the chief accompanied British Indian agent Matthew Elliott who followed in the army's wake. He rejoined Indian forces near modern Chatham, Ontario, where on October 4 he was wounded when the warriors half-heartedly opposed the advance of American forces at McGregor's Creek. On the following afternoon he was killed after British troops fled from Harrison's army at the Battle of the Thames. Although Sugden does not specifically state that Richard M. Johnson killed Tecumseh, he illustrates that the Kentuckian most probably was responsible for the Shawnee's death. He also indicates that, when the fighting ended, Tecumseh's body was first mutilated and then buried adjacent to the battlefield.

In delineating these events, Sugden discusses the myriad conflicting accounts focusing on Tecumseh's death and the fate of his body. Indeed, perhaps the volume's major contribution is Sugden's dismissal of much of the apocryphal information regarding the Battle of the Thames and its aftermath. After almost one hundred seventy-five years these events cannot be reconstructed with perfect certainty, but Sugden's discussion offers the most logical and detailed analysis of these events that has yet appeared.

This volume is an excellent example of a relatively limited series of events thoroughly investigated. It is well written and should be welcomed by scholars interested in the War of 1812 and native American history.

R. DAVID EDMUNDS
Texas Christian University

IRA ROSENWAIKE. *On the Edge of Greatness: A Portrait of American Jewry in the Early National Period.* (Publications of the American Jewish Archives, number 14.) Cincinnati, Ohio: American Jewish Archives. 1985. Pp. xvi, 189. $25.00.

We owe much to conscientious historical demographers such as Ira Rosenwaike, whose *Population History of New York City* (1972) is of lasting value for urban and ethnic history. In his present work Rosenwaike uses his skills to wring from the census of 1830 a statistical profile of American Jewry before the great migration of German Jews, who in the next decades were to alter the character of Jewish life. This work exceeds simple compilation, since the census provides no clues regarding religion. By using various public and religious records, Rosenwaike has succeeded in identifying 629 Jewish households with some four thousand members, about whom he has fashioned fifty statistical tables relating to such matters as the origins of household heads (a substantial majority were foreign born), age and sex ratios (roughly similar to those of urban Gentiles), the number of Jewish slaveholders (a majority of the 322 southern households that Rosenwaike studied owned slaves), and the occupations of Jews in the major cities.

From this data Rosenwaike reaches four conclusions about American Jews in the early national period. First, previous estimates exaggerated the size of the Jewish population at this time by approximately 50 percent. Second, by 1830, Jews had already begun to concentrate in New York and Philadelphia rather than in the southern port cities. Third, even before 1830, English and Dutch Jews predominated over the Sephardim, a significant anticipation of the Ashkenazi migration from Germany. Finally, this small population secured a place and created the institutions that eased the entry of later Jews into American society. In sum, the outline of the modern Jewish community had already been drawn by 1830.

This work has some of the weaknesses of purely demographic studies. Rosenwaike's dedication to statistics, for instance, yields a rather lifeless picture of the Jewish community, which is only feebly animated by his occasional references to conventional history and his inclusion of a dozen portraits of prominent Jews. Moreover, the apparent precision of his figures is somewhat doubtful. This problem, common to statistical studies, is compounded by the shortage of records needed to identify all members of the Jewish population, especially in places without formal congregations. To resolve this problem, the author increased his count by 10 percent, a reasonable procedure but one that cautions about the accuracy of its results—particularly when the author is able to list only 4 Jewish households for all

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of New England. If used with due care, however, this can be a book of enduring value, a work that should elicit much gratitude for Rosenwaike's dedication and skill.

EDWARD K. SPANN
Indiana State University

JOHANNA NICOL SHIELDS. *The Line of Duty: Maverick Congressmen and the Development of American Political Culture, 1836–1860*. (Contributions in American Studies, number 80.) Westport, Conn.: Greenwood. 1985. Pp. xix, 297. $37.50.

How are we to appraise the public rhetoric of major party politicians in an age of patent demagogy? American political historians have offered diverse answers to this classic question—some taking antebellum political oratory at close to face value, others dismissing it as claptrap, and still others insisting that, if it cannot be taken literally, it must yet be taken seriously. Johanna Nicol Shields identifies with the last school of thought, and her book is a thoughtful, informed, and original contribution to the discussion of her significant theme. This contribution is all the more welcome because the theme, for all its importance, has been somewhat neglected in the years since Marvin Meyers and Lee Benson focused attention on it.

Shields has had the interesting idea of examining the public rhetoric and the published writings of more than two hundred fifty major party politicians who were members of Congress during the quarter-century before the Civil War in order to ascertain the differing values of what she calls conformists and mavericks and the light these differences throw on American society and "political culture." (Her "conformists" regularly voted the party line; her "mavericks" regularly deviated. Shields, of course, offers statistically precise measures of the two categories.) Since Shields is interested, too, in unearthing the motives and inner feelings of her subjects, her almost total reliance on published materials is, as she is aware, a weakness that compels her at times to fall back on conjecture and surmise. But she argues effectively, if not altogether convincingly, that the recorded data on which she does rely is more germane than other sources of information. She has also read widely in the relevant sociological, psychological, and political science literature the better "to integrate the concrete detail usually associated with classical historical studies and the functional abstractions about legislative behavior resulting from political science research" (p. xvi).

Although mavericks constituted less than one-third of the congressional total, Shields devotes most of her attention to them, not least because they were more interesting and had much more to say than did their more compliant colleagues. Although not always persuasive, Shields's judgments and interpretations are unfailingly interesting, whether about the differences between conformists and mavericks, Democrats and Whigs or Republicans, northerners and southerners, or about the limits of quantitative research and statistically oriented methodology.

That Shields is invariably sensible and judicious in treating issues does not mean that her treatment is free from fault. She never quite demonstrates that, through Thoreau's "imaginative perspective, the cultural tensions centrally involved in the character of mavericks may be elucidated" (p. 11). She discerns "romanticism" in commonplace political clichés about the sovereignty of God and "sensitivity to romantic currents in American culture" in the clever use made by some politicians of their reading (p. 159). She is at times forgetful that her mavericks are mavericks only in a manner of speaking, that their occasional departures from party regularity not only promoted their own political careers, as she points out, but also proved profitable to the pragmatic parties that so regularly permitted these deviations. And her case for the mavericks as barometers of complex social change is not compellingly made.

Yet, on balance, this book is a stimulating addition to the literature. Not the least of its virtues are the many nuggets of fascinating factual data sprinkled throughout the text on famous men such as John Quincy Adams, Alexander H. Stephens, and John Pendleton Kennedy and on the many more men who are unknown or only slightly known, as well as on antebellum politics and other aspects of American life and culture.

EDWARD PESSEN
Baruch College and
Graduate School and University Center, City University of New York

MICHAEL GROSSBERG. *Governing the Hearth: Law and Family in Nineteenth-Century America*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1985. Pp. xiv, 417. $32.00.

This book makes a major contribution to American legal history and the history of the family. It is distinguished by the author's mastery of the scholarship in both fields and by his integration of the critical issues in that scholarship into his primary research. Michael Grossberg not only fills a serious gap in nineteenth-century legal history by infusing it with a broad social and familial dimension but also brings a significant legal dimension to the history of the family. As he astutely observes, to some extent the modern, companionate family that has preoccupied social historians might be viewed as a legal

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construction shaped by the judiciary during the formative era of American law.

Locating the formative era of family law in the late eighteenth and early nineteenth centuries, Grossberg identifies its central tenets as minimal state interference in the governance of families and enlarged judicial recognition of individual family members. In contrast to the patriarchal concept of the family as "a little commonwealth," emerging republican law gradually conformed to a republican familial ideal by treating wives and children as discrete, semidependent legal personalities. According to Grossberg, republican components underpinned American family law throughout the nineteenth century and were carried into the twentieth. By the turn of the century, however, new forms of judicial intervention and control modified significantly the republican paradigm and reshaped it into a "judicial patriarchy." Thus, the emergence of the term "domestic relations law" marked the legal consolidation of formerly fragmented areas of law and the judicial domination of once-private areas of family rule.

In a thesis similar to that of the French sociologist Jacques Donzelot, Grossberg argues that, whereas the family had once stood as a buffer to the power of the state over its individual members, the state, through the institution of the judiciary, increasingly breached the barriers of the family, especially with respect to the dependent poor. To oversimplify what is a complex and sophisticated argument about the distribution of power within society: the male head of household gradually lost his sole authority within the family to his wife and children, who were put under the partial tutelage of the judiciary. Ultimately, the judiciary itself became the final arbitrator in matters of domestic relations.

At the core of Grossberg's research are state appellate court decisions on selected strands of family law; these include changing rules of courtship, regulations regarding marriage, restrictions on contraception and abortion, definitions of bastardy and legitimacy, and innovations in child custody and adoption. In a well-written and readable text he illustrates these trends with individual cases, each of which have been culled from extensive research that is meticulously footnoted. Some readers, however, will undoubtedly question Grossberg's topical choices. Why, for example, include breach-of-promise suits and exclude divorce? Nevertheless, there is logic and historicity to his selections, for his focus is on the shifting legal delineation of family formation and governance rather than on its dissolution, and breach-of-promise suits illuminate changing notions of betrothal and contract. As one might expect, the historical actors who come to life on these pages are not members of the family but judges; their prominence is an integral part of his thesis. Judges are at once the stars and villains of the whole enterprise called domestic relations law.

Grossberg's emphasis on the judiciary's biases and penchant for power is bound to spark some controversy, if only because it is applied so comprehensively to the development of family law. At times, moreover, he seems to fit his evidence almost too neatly into his larger thesis. Still, it is a thesis developed with extraordinary skill and erudition. Clearly, he has constructed a sturdy bridge between legal history and the history of the family, a bridge that is likely to be used by students and scholars in legal history for some time to come.

NORMA BASCH
Rutgers University

JOHN F. STEGEMAN and JANET A. STEGEMAN. *Caty: A Biography of Catharine Littlefield Greene*. Foreword by HARVEY H. JACKSON. (Brown Thrasher Books.) Reprint. Athens: University of Georgia Press. 1985. Pp. xxiii, 235. Cloth $25.00, paper $9.95.

If this biography of Catharine Littefield Greene (1753–1814) is anything, it is a good story. John F. Stegeman and Janet A. Stegeman have produced an engaging biography of a most engaging heroine. The book traces Caty's progress from a sheltered and isolated girlhood in Rhode Island to her advantageous marriage to farmer-merchant Nathanael Greene in 1774. The American revolution dramatically altered what might have been a normal upper-class life spent running a household, raising children, entertaining, and acting as dutiful companion, for a year later Caty became the consort of one of the leading patriot generals. If she found the trials of running a home and bearing children alone difficult, she shone brilliantly in the frenetic social whirl that accompanied the patriot officers' winter encampments. Afterward, as a reward for Greene's service in the southern campaign, she became mistress of a Georgia rice plantation. After the general's death in 1786, Caty would spend much of the remainder of her life trying to recover the family's fortune (lost in part by Greene's assuming personal responsibility for provisioning his troops during the war). Caty had always been more adept at gaining the friendship and support of men than she was at pleasing other women. Influential male contacts supported her through the trials of widowhood, and she was so loath to surrender her new-found autonomy that for ten years she substituted affairs for remarriage. To the end Caty remained active in financial and political circles, and her sponsorship of Eli Whitney's work on the cotton gin was a major contribution to technological progress.

This is the stuff of historical romance. The book, however, is based on extensive research in Greene

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family correspondence, and, where gaps had to be filled imaginatively, the results read convincingly. The authors, self-described "amateur history seekers" (p. xxi), intended to write a popular book. Harvey H. Jackson's foreword to the paparback edition helps place the biography in the context of recent scholarly interpretations of the role of women in early America. Unfortunately, this cannot remedy a lack of social context, especially where the lower orders are concerned. Granted, Caty was intimate with many of the leading figures of the time and spent most of her life in easy circumstances. But, after coming to the South burdened with doubts about the slave system, was she able to dismiss her blacks as nothing more than passive chattels as easily as this narrative would suggest?

This book illuminates some of the more interesting aspects of the story of a woman who took an active part in America's revolution. It ought to be read, however, in conjunction with the much more socially perceptive biography of Mary Fish (1736–1818), *The Way of Duty* (1984) by Joy Day Buel and Richard Buel, Jr. Fish grew up in Caty's neighborhood and shared similar wartime experiences. The Buels' fuller portrayal of Fish's total social world supplies a better context from which to evaluate Caty's experiences.

LORENA S. WALSH
Colonial Williamsburg Foundation

DAVID T. BAILEY. *Shadow on the Church: Southwestern Evangelical Religion and the Issue of Slavery, 1783–1860.* Ithaca: Cornell University Press. 1985. Pp. 293. $24.95.

This treatment focuses on Kentucky, Tennessee, Mississippi, and Alabama to 1860, where David T. Bailey found thought and behavior patterns markedly divergent from those ascendant in adjoining Atlantic Coast states. Indeed, these patterns were distinct enough to lead Bailey to categorize the region as one of seven major subcultures in early America. Religious zeal was one identifying trait of the Old Southwest, as were degree of antislavery sentiment vis-à-vis the Southeast and frequency of contentions among churchmen about slavery.

Bailey believes that the influences of the northeastern churches were more pervasive among southwesterners of that era than among southeasterners and that the slave system may have burdened the consciences of some who accommodated to it without protest. On the one hand, he minimizes early nineteenth-century color discriminations in biracial Christian fellowships, declaring that "before about 1830 blacks were in many ways on an equal standing with whites in the day-to-day activities of the [mixed] congregations" (p. 181) and alluding to a dearth of indications that co-worshiping blacks and whites customarily sat in separate areas of sanctuaries prior to 1820 (p. 192). On the other hand, he notes that religious missions among slaves were apparently less a concern among whites in the Old Southwest than among whites in the Old Southeast and that racial attitudes hardened in time. He elaborates on the extent to which churches and churchmen mirrored the encompassing temporal order rather than shaped it. As he acknowledges, his book turns out to be a story "occasionally ugly and always very sad" (p. 24).

Although this study poses no wholesale challenges to previous interpretations, it deserves the careful attention of those with historical interests in the American South, the American frontier, and American religion. It rests on comprehensive research (much of which was in obscure and scattered sources), is sensibly organized, and offers solid conclusions. Unfortunately, the book has an extraordinary number of editorial flaws as well as occasional serious noneditorial oversights. Everett Dick's classic, *The Dixie Frontier* (1948), is unmentioned, for example, and another landmark, John B. Boles's *The Great Revival, 1785–1805: The Origins of the Southern Evangelical Mind* (1972), is consistently miscited as *The Great Revival in the South.* The author admonishes students of Old Southwest religion to start "with the works of Walter Posey" (p. 264) but himself ignores Posey's best-known book, *Frontier Mission: A History of Religion West of the Southern Appalachians to 1861* (1966), as well as another standard work by Posey, *The Baptist Church in the Lower Mississippi Valley, 1776–1845* (1957).

KENNETH K. BAILEY
University of Texas, El Paso

DEBORAH GRAY WHITE. *Ar'n't I a Woman?: Female Slaves in the Plantation South.* New York: W. W. Norton. 1985. Pp. 216. $18.95.

Deborah Gray White describes the lives of antebellum black women in terms of three major impediments: sexism, racism, and slavery. Her study, she explains, is limited by the unavailability of sources. Because few manuscripts generated by female slaves have been unearthed, she relied heavily on such materials as the Works Progress Administration interviews with female former slaves.

White explores the issue of sex and race, explaining the perception of many southern whites that miscegenation occurred because, though sometimes feigning resistance, black women invited sexual overtures from white men. In addition, she describes the opposition of white women to miscegenation, although they could do little about it. At the

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same time that they characterized black women as Jezebels, whites praised black women's efficient care of white charges and their role as the mammy of the household. Both images were misleading, although evidence for the stereotypes could be found in the roles black women were required to play.

According to White, the preponderance of male over female slaves made it easy for females to find mates, even when forced separations made many women household heads. Childbearing and work in the fields were the major tasks for most black women. The author finds no convincing evidence of birth control and abortion. Infanticide was atypical. Despite heavy labor and less nutritious diets, black women's fertility rate was higher than that of southern white women, and pregnancy was one way of avoiding work for a time. She concludes that the most important distinction between male and female bondage was that the slave women's childbearing and child care responsibilities reduced their involvement in resistance activities and decreased their numbers in the ranks of fugitive slaves. Female slaves were more likely to become truants for a few days. On occasion, women did violently resist sexual exploitation and used poisoning as well as feigning illness as modes of resistance.

Relying heavily on the work of Herbert Gutman and John Blassingame for her discussion of black families under slavery, White describes the general pattern of the female slave's life cycle. In childhood she was a nurse for younger children, a task shared with boys. By age twelve she began field work. At first, girls worked in gangs consisting only of pregnant and nursing women and the very young and very old. In these gangs, girls received their first acculturation into women's sex roles. Usually female slaves had their first child at nineteen, two years before southern white women. Masters encouraged mating and "marriage" and rewarded prolific child bearers. Infertile women were quickly sold off, if possible. But slaves took marriage seriously and tried to protect family life as much as possible, using it as a means of transmitting their culture.

Social status increased with old age, and female slaves usually ended as they began—caring for the children of parents who worked in the field. They also served as midwives and folk doctors, relying on a knowledge of herbs they had learned over the years. Female slaves, White asserts, grew up believing boys and girls were equal. "Had they been white and free, they would have learned the contemporary wisdom of nineteenth-century America" that women were delicate. But, "because they were black and slaves, they learned that black women had to be the maid servants of whites but not necessarily of men" (p. 18).

In a chapter on the female slave network, White describes how adult female cooperation and interdependence was a fact of female slave life. Men plowed and women hoed; women also did domestic work such as sewing and weaving, and they worked long after they returned from the fields and long after the men had retired. Females cooperated in the delivery of newborns and in convalescent and child care. Women who were midwives, folk doctors, cooks, and seamstresses had superior status, as did work-gang leaders. Those who were especially good at any particular task gained the respect of the others.

In an overly ambitious epilogue Whites traces the effects of victimization in the period since abolition. She discusses the disproportionate number of black women in the work force, how a "culture-based condemnation of illegitimacy never developed in black society" (p. 164), and how Jezebel and Mammy were joined by Sapphire, who "emasculates men by the aggressive usurpation of their role" (p. 167).

White's book is a much-needed description of the lives of female slaves and should stimulate additional, more narrowly focused studies. More work in less obvious sources such as court records should provide additional information concerning the day-to-day life of female slaves in particular localities in the American South.

MARY FRANCES BERRY
Howard University

MARY V. DEARBORN. *Pocahontas's Daughters: Gender and Ethnicity in American Culture*. New York: Oxford University Press. 1986. Pp. 266. $21.95.

This book is neither a history of American ethnic women nor a systematic literary study. Rather, it is a topically organized essay about native American, black, and immigrant women as writers of fiction in the United States in the nineteenth and early twentieth centuries. Because of their ethnicity and gender, these writers are doubly "marginal" to the Anglo-Saxon, Protestant, male literary tradition. Mary V. Dearborn argues convincingly, however, that their very marginality makes these writers central to an understanding of the United States, where everyone was an immigrant and culture is permeated with problems of ancestry and identity.

Dearborn introduces obscure writers such as Mourning Dove, native American author of *Co-ge-we-a: The Half Blood* (1927), and Harriet E. Wilson, the first known black woman novelist, and provides new insights about familiar figures such as second-generation Jewish writer Gertrude Stein. Dearborn's interest, however, is not in individual writers and ethnic groups but in common experiences and perceptions. She notes, for example, that many ethnic women had problems establishing the authenticity of their authorship because they often

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entered the literary scene with the help of mainstream "sponsors"—friends, editors, public school teachers.

Dearborn shows that women from many ethnic groups used literature as a form of mediation, interpreting minority culture for mainstream America and trying to defuse conflict by explaining differences and stressing similarities between the groups. Such mediation was the major purpose of the "Reconstruction novels" by black novelists Frances Harper and Pauline Hopkins. On one level, these novels written after the Civil War were addressed to newly freed blacks, urging them to be loyal citizens and to strive for middle-class respectability. On another level, however, they were addressed to whites, condemning their exclusion and mistreatment of blacks and thus protesting rather than reinforcing the status quo. This double perspective of the "trickster" appeared frequently in texts written by ethnic women.

Other common themes were intermarriage and miscegenation. In many of these novels marriages took place between ethnic women, who were portrayed as passionate, emotional, and sexual, and mainstream men, who were portrayed as wealthy, cold, and rational. For the women, marriage was a way of laying claim to the New World; for the men, it was compensation for personal and cultural deficiencies. These relationships were usually ill fated, as Dearborn illustrates with the example of Jewish immigrant author Anzia Yezierska. Yezierska's real-life love affair with John Dewey, like the relationships between foreign-born women and native-born men in her fiction, was short-lived. The mainstream male is ultimately repulsed by the very qualities—warmth and sexuality—that attracted him to the ethnic woman in the first place.

Among black authors miscegenation is an important theme, and the mulatto, especially the one who can pass for white, is a recurring character. The mulatto is a symbol of ultimate marginality, of disinheritance, of the differences between blacks and whites, and of their common humanity. Through novels about intermarriage and miscegenation, ethnic women explored a major preoccupation in American culture, the conflict between Americans as self-made individuals and their simultaneous obsessive search for ancestry.

This book provides valuable information about writers not usually included, much less honored, within the American literary canon. Its most important contribution, however, is its insightful use of ethnic women's literature to explore the dilemmas of identity in a pluralistic society not yet comfortable with its diversity. Interdisciplinary in the best sense, *Pocahontas's Daughters* will be of interest to scholars in literature, women's studies, ethnic studies, and social and cultural history.

MAXINE S. SELLER
State University of New York, Buffalo

CARROLL SMITH-ROSENBERG. *Disorderly Conduct: Visions of Gender in Victorian America.* (Borzoi.) New York: Alfred A. Knopf. 1985. Pp. viii, 357. $19.95.

Carroll Smith-Rosenberg's anthology of essays is careful, scholarly, and radical in its insistence on women's centrality in history, its refusal to accept gender roles as natural, its interdisciplinary view of gender as the "most fundamental ordering of . . . power arrangements within the society" (pp. 12–19). The anthology contains six of Smith-Rosenberg's previously published essays and new ones as well—most important, "The Abortion Movement and the AMA" and "The New Woman as Androgyne." Juggling an impressive array of interdisciplinary methods, and at the same time forging new hypotheses herself, Smith-Rosenberg unquestionably stands as one of the most prominent scholars in the field of women's studies.

Smith-Rosenberg examines the ways in which socioeconomic instabilities and psychological anxieties express themselves in language. As the nineteenth-century revolutions in commerce, transportation, and industry inspired disruptions and confusions, dominant males constructed ideologies to contain their fears and legitimize their powers, whereas "disorderly" women used language to protect themselves. Historians have usually attended to the public language, but Smith-Rosenberg looks at the more private women's view as well. She sees a conversation rich with body metaphor and imagery, often a fierce and hate-filled one, primarily because the family itself was enduring massive socioeconomic change. As people felt threatened by the unfamiliar and insecure within their changing family roles, they responded by attempting to control sexual behavior, which in fact meant trying to control their socioeconomic world.

Smith-Rosenberg offers a "social structural" decoding of nineteenth-century metaphor. She views language as a social mirror as well as a social agent, a demonstration of men and women using sexual imagery to reflect their experiences and expand their power. Male images of women are, of course, more familiar: the sinful prostitute, the man-hating lesbian, the selfish mother who aborts a child. Unfortunately, we may occasionally forget the underlying life-and-death realities such images reflect. For example, they provided the rationale for laws forbidding the distribution of sexual information; they legitimized antiabortion legislation, which in turn

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promoted black-market abortions that annually took the lives of thousands. Whatever the specific issue, Smith-Rosenberg explains the process by which "metaphoric language is politicized, collective fears are transformed into collective actions, and the sexuality of others is regulated" (p. 224).

Although it is discomforting enough to hear the male side of the sexual dialogue, some will find it even more so to hear of women's "disorderly" response. For, as women faced their own fears and insecurities, they were usually not inclined to blame themselves. They focused instead on marital rape, on deserting husbands or unwanted pregnancies, on commercialized sex as a symbol of abusive power, on the friendless prostitute as a symbol of the powerlessness they felt themselves.

Despite Victorian prescriptions that women should be passive, pure, and pious, women themselves used sexual metaphors to describe their experiences and protect their lives. Smith-Rosenberg celebrates their efforts. She shows how women of the Female Reform Society managed to invade bordellos and befriend harlots in public protest against rape and prostitution; how women of the Second Great Awakening used antiritualistic rhetoric with "radical" intent; how "bourgeois matrons" twisted "true womanhood" prescriptions to promote social justice; and how, by the late nineteenth century, "respectable" and "genteel" ladies supported unions, formed unemployment bureaus, founded settlement houses, and developed life-style alternatives and female-centered friendships that fostered their creative growth.

Although many nineteenth-century women managed to subvert contemporary language and thus affirm their own perspectives, Smith-Rosenberg describes one group whose "brave new vision" failed. The "new women" of the 1920s—short-haired, cigarette-smoking, sexually experimental—were apparently too threatening. Boldly asserting their right to professional, political, and sexual equality, they tried "to enter the world as if they were men" (p. 295). Like Virginia Woolf's *Orlando*, they gloried in the gender chaos, in extending options beyond proprieties, in challenging gender roles that for so long twisted and deformed the human spirit.

Not surprisingly, the sexual dialogue of the 1920s became even more hate-filled and brutal than it had been before. When rebels started using dress and body imagery to repudiate gender definitions, men started attacking their behavior as perverse. And, whereas earlier women had enjoyed affectionate and supportive female friendships without attracting censure, by the 1920s a common way to denigrate women reformers was to speak about their homosexual inclinations, their unsatisfied libidos, their guilt for "unnaturally" and selfishly rejecting men. Social ostracism, legal censorship, and public condemnation followed. And no wonder. The "new woman" of the 1920s had not only opened the door for further protest by challenging the conventional image of "natural" sexuality but also, by pushing for creative sex-role options, challenged the distribution of power itself.

In evaluating the failure of the 1920s rebels, Smith-Rosenberg confronts a classic female double bind: how to attain autonomy and respect in the masculine arena while still affirming the values and traditions of the female past. Because of their marginality, women had unwittingly and often self-destructively used "masculine" metaphor and language, albeit twisting its meaning and subverting its intent. But not until the 1920s were the dangers inherent in the strategy so clear. On the one hand, by adopting "masculine" symbols, they risked alienating other women, rejecting aspects of their own "feminine" identity, and also relinquishing the power to create metaphor and language for themselves. On the other hand, as Carol Gilligan has suggested in another context, women speaking "in a different voice" are rarely heard. As long as the standard criteria of value and significance continue to be masculine, affirmations of female language and tradition almost never secure public power or respect.

Smith-Rosenberg, prominent in women's studies, has faced this double bind herself: to use or not to use the traditional (masculine) academic style. On the one hand, she has risked alienating nonacademic women by using formal, distant, abstract argument; on the other, she risked writing articles the academic world ignores. Yet, since she says herself that men and women of the past had problems understanding one another—neither "could hear or respond to the others' fears" (p. 244)—is she therefore wise to present her message indirectly? Are her arguments unnecessarily obscure? Alternately, might indirectness be more useful for presenting issues so potentially explosive? Given continuing sexual fears and conflicts, to say nothing of male privilege and power, do we still need metaphoric systems that can cloak underlying meanings? Do we still need nonthreatening language "so that what is too conflicted to be spoken directly can nevertheless be said" (p. 18)? Does distance and formality give argument more bite?

Ultimately, Smith-Rosenberg takes a stance consistent with the nineteenth-century heritage. She uses, and then subverts, traditional (male) academic prose. By offering a tightly argued, richly woven, sometimes dense and convoluted argument, she excites her readers to serious intellectual engagement, thereby provoking them to consider her disorderly, radical intent. Elegantly, if not quite directly, she explains her goal: "Words are active as

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well as reflective agents. When heard and understood, they can revolutionize their world" (p. 296).

MARY ARMFIELD HILL
Bucknell University

DOLORES E. JANIEWSKI. *Sisterhood Denied: Race, Gender, and Class in a New South Community.* (Class and Culture.) Philadelphia: Temple University Press. 1985. Pp. xvii, 248. $34.95.

In this slender volume Dolores E. Janiewski sets herself a formidable task, the exploration of "the appeals for racial, gender, and class solidarity as competing forms of group and self discovery" among the developing work force of Durham, North Carolina (p. 6). This emphasis on gender, the author believes, should benefit southern history, which heretofore has ignored gender to focus instead on divisions of class and race. By examining gender, Janiewski contends, historians can better comprehend "the obstacles that hindered the agrarian and labor movements in the South" (p. 6).

As an effort to integrate the activities of women into the history of the New South, this book succeeds admirably. Through the use of both oral history and more traditional sources, Janiewski skillfully describes the hardships endured by women, both on the farm and in the factory. As the production of cash crops, either cotton or tobacco, came to dominate the region's agriculture, women—their labor less valued than that of men—were more quickly driven from the land. Among the growing tenant population, women bore the brunt of the hardships imposed by a stagnant agrarian economy. Expenditures for home and household items by both landholding and tenant families in the 1920s, for example, averaged just 3 percent of the family budget, whereas expenditures for farm implements consumed one-third of the tenant's budget and nearly one-half of the budget of the landowning farmer. Labor-saving devices for women were invariably the last items purchased by farm families.

Employment in Durham's rapidly expanding tobacco, textile, and hosiery industries did not alter women's subordinate status. In all three industries the better-paying skilled and supervisory positions went to males; women received the less-skilled, low-wage jobs. Black women remained at the bottom of the economic ladder, employed primarily as stemmers in the tobacco factories. Both black and white women endured sexual exploitation by male supervisors, and both were charged with running the home in addition to performing their "public work." After a ten-hour day in the factory, women returned home to the domestic chores of cleaning, cooking, and child rearing. Well into the 1930s, every waking hour saw a demand on most factory women's time.

Less convincing is Janiewski's contention that gender was a crucial factor in developing class consciousness among Durham's new industrial proletariat. The author briefly chronicles the rise of unions in Durham in the 1920s and 1930s, a period in which tobacco workers achieved some solid success while textile operatives won only temporary victories. Even such modest achievements, Janiewski contends, could not have occurred without active female participation in the labor movement. She fails, however, to show that female workers responded differently to their economic circumstances than did their male counterparts.

In fact, Janiewski's evidence strongly suggests that women did not respond differently. Like males, women deserted the farm for the promise of the better life the factory offered. A surplus labor supply forced males and females to take jobs offered at wages determined by the employer. Women, like men, accepted the racial divisions within the society. White women who ran cigarette-making machines, for example, expressed little concern for the lower-paid black women who separated tobacco stems from the leaf. There is also no evidence to suggest that a greater percentage of women than men supported unionization. Women seem to have reacted to unions as did men: some joined and fought for economic justice; others shunned the union for fear of losing their jobs.

Despite its ambiguity about the role of gender in shaping class consciousness, an ambiguity acknowledged by the author, this book enhances our understanding of the role of women in the industrial work force of the New South. And the quotations taken from the oral histories of women who composed that work force eloquently convey "their pride in having survived hard times and tribulations" (p. 178), even if they do not convince us that gender considerations motivated the women's economic actions.

MELTON A. MCLAURIN
University of North Carolina, Wilmington

GERALD W. MCFARLAND. *A Scattered People: An American Family Moves West.* New York: Pantheon. 1985. Pp. xxii, 280. $17.95.

This book begs for comparison with Nicholas Perkins Hardeman's *Wilderness Calling: The Hardeman Family in the American Westward Movement, 1750–1900* (1977). Both books trace several generations of ancestors (Hardeman four and McFarland five), which were active participants in the westward movement. Both books are welcome additions to western history, and both raise the question of why more historians with such ancestry have not written

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about their families. Comparison of the two studies, both of which are capably and professionally written, reveals that the experiences of the two groups were substantially different. The Hardemans retained leadership qualities and an upper-middle-class image. The family that concerns Gerald W. McFarland—with branches named Brown (including the John Brown of Harper's Ferry), Root, Remington, Ward, Greenfield, and Adair—is much more disparate and less successful. From the many ancestors dating back to the 1760s he has chosen those who, for one reason or another, he believes are worthy of mention.

McFarland chose branches from different geographical locations. In Ohio, his New England and New York relations merged with Scotch-Irish folk from southwestern Virginia. He sees them involved in the issues of the time while succeeding or failing in their vocations of business, farming, and the ministry. In a particularly readable segment of the book the author deals with Oberlin College, the abolitionist movement, "Bleeding Kansas," and John Brown.

McFarland's ancestry raises questions about the validity of the American dream. That image included the conviction that the children would fare better than their elders, that possibilities for betterment in the West were always more attractive than what was offered closer to home. But the reality, at least insofar as the McFarland descent was concerned, was far removed from the image. Although a few ancestors—notably, a man named Jerry Remington—elevated their status to upper middle class, most of the McFarland ancestors failed to elevate themselves at all.

McFarland discusses this failure. In his family he finds "dreams of purity or dreams of plenty" (p. 239): the former refers to a family strain of religious dedication verging on fanaticism whereas the latter implies success in the marketplace or on the farm. For most of the McFarland ancestry, the promise of American life was larger than the accomplishment. Generation after generation, it turns out, remained in the lower strata of the great American middle class. So too, presumes the author, were countless other family lines involved over several generations in the westward movement. Thus, the final impression one gets from reading this book is one of futility and even sadness for the plight of mankind: so many high hopes, so few achievements.

In his well-researched and well-organized book that is permeated with a strong sense of history, McFarland has made a useful contribution to the history of the westward movement. Some of his comments are homey and earthy. Parts of his narration, especially segments dealing with women left as widows with several children, arouse pity. Photographs, a map of the ancestors' travels, a genealogical chart, and adequate documentation all add to the quality of this book.

RICHARD A. BARTLETT
Florida State University

GORDON MORRIS BAKKEN. *The Development of Law in Frontier California: Civil Law and Society, 1850–1890.* (Contributions in Legal Studies, number 33.) Westport, Conn.: Greenwood. 1985. Pp. 162. $29.95.

This small book by Gordon Morris Bakken investigates fundamental legal questions about the development of California society from 1850 to 1890. The chapters cover contract law, landlord and tenant law, mortgage law, tort law, admiralty law, and the financing of commerce. Much of the book originally appeared in the *Southern California Quarterly.*

Justices of the California Supreme Court often rendered opinions without citing authorities, and contracts were susceptible to interpretations without precedent. On occasion, the court considered questions of constitutional law in real estate transaction cases. In *Floyd* v. *Blanding* (1879) waterlot warehouse owners of San Francisco challenged the plan of the Board of State Harbor Commissioners to build a sea wall that would limit their business and profits. The court granted to the state the power to alter the waterfront for its own commercial advantage at the expense of the warehouse owners' profits.

Written contracts were important in trade, and the court called on parties to make explicit the particulars of transactions if they wanted judicial enforcement. Since money was scarce in the frontier economy of California, promissory notes circulated as currency. Bankruptcy laws were lenient, and California became a haven for debtors. Building contractors, moreover, often found that cities and towns avoided payments. Initially the state supreme court had a prolonged problem of legally defining a mortgage, but by the 1880s mortgage law finally matured and served the marketplace better. The growth of industry and railroad expansion increased tort litigation, and the court favored the employer in liability cases. The court viewed its admiralty jurisdiction as extending to all subjects of maritime commerce. In *Gunter* v. *Geary* (1851) Chief Justice Serranus C. Hastings asserted the absolute right of a state to regulate and improve the navigable waters within its jurisdiction.

Chapter notes are thorough and based largely on law cases and manuscripts; the brief bibliographic essay, however, is inadequate and has minor errors. One oversight in the chapter on contract law is the failure to mention the Specific Contract Act of 1863, passed by the California legislature, that virtually

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nullified the federal law authorizing greenbacks. At times, the writing style is abstruse and ambiguous, but overall the author has made a substantial contribution to a neglected aspect of California history.

BENJAMIN F. GILBERT
San Jose State University

JESSIE L. EMBRY and HOWARD A. CHRISTY, editors. *Community Development in the American West: Past and Present Nineteenth- and Twentieth-Century Frontiers.* (Charles Redd Monographs in Western History, number 15.) Provo, Utah: Charles Redd Center for Western Studies. 1985. Pp. viii, 237.

The title of this book is misleading. It is an anthology of selections written primarily by scholars from Brigham Young University, including authorities in literature, anthropology, sociology, and history. Except for a chapter on Phoenix business leaders, the authors deal primarily with community developments relating to the Church of Jesus Christ of Latter-day Saints. The editors, Jessie L. Embry and Howard A. Christy, fail to explain the purpose of the collection in their eight-hundred-word introduction. The closest Embry and Christy come to telling what they are about is when they note that change is underway in the late twentieth-century West: "The West is now facing a new frontier. With the development of new energy sources in the region, towns and cities that were almost in the process of dying are now growing rapidly" (p. vii). What follows contributes to the history of western community building.

Perseverance in the face of continual problems emerges as a general theme. A number of the thousands of communities founded in the course of western settlement overcame early failures to succeed in recent years. Discussing human issues in the evolution of western society, anthropologist John L. Sorenson uses modern Utah as a case study to show the importance of outside forces in formulating economic policy. He wonders whether westerners have any conception of what they do not want to do in building their region. Stan Albrecht, a sociologist, indicates that many older communities that boomed in the 1970s because of decisions made by the federal government and large outside corporations should be studied by other western cities to determine if they too want to encourage energy-related growth. English professor Edward A. Geary shows how Huntington, Utah, which underwent a conventional Mormon frontier experience, slowly declined after 1895 until an energy boom in the 1970s brought renewal and dramatic growth. Historian G. Wesley Johnson emphasizes the importance of changing combinations of boosters in pushing Phoenix ahead to the status of a major American metropolis. Another historian, Stanley B. Kimball, demonstrates how his relative, Mormon leader Heber C. Kimball, greatly complicated his financial problems by having "plural wives." After studying frontier community patterns in Spring City, Utah, Michael S. Raber, a consulting anthropologist, concludes that individual householders had a greater impact than did Mormon church organizations. Embry, an oral historian, reaches an opposite conclusion. He supports the traditional view that the Mormon church played a major role in urban Utah until well into the twentieth century. Leonard J. Arrington, a noted western historian, indicates the broader contributions of Mormons in the West, especially in railroad building. Historian Larry R. Gerlach uses an 1883 lynching of the alleged killer of a Salt Lake City police chief to show how thin a line separated savagery and law on the frontier. Gerlach's account and those of the other contributors provide an interesting and eclectic view of western community building.

These well-researched and cross-disciplinary selections are welcome additions to the history of the West. Of special note are those that bridge the frontier period and delineate late twentieth-century growth patterns. Unfortunately, the book fails to stand as a whole, despite the high quality of the individual chapters.

LAWRENCE H. LARSEN
University of Missouri, Kansas City

DOUGLAS COLE. *Captured Heritage: The Scramble for Northwest Coast Artifacts.* Seattle: University of Washington Press. 1985. Pp. xiii, 373. $17.50.

This history traces the progress of artifacts through the hands of various owners—those who made them, those who collected them, those who bought them, and those who wanted them returned. The approach is novel, to say the least, and the scheme is brilliantly conceived and skillfully executed. In general, this work tells us a great deal about native history for the Northwest Coast of North America in the nineteenth and twentieth centuries; in particular, it goes beyond the usual policy studies and cross-cultural research in which historians are commonly engaged.

Douglas Cole's work is an exercise in tracking down a native American material culture that was born or energized in a newly established pattern of trade and then widely distributed to New York, Washington, Chicago, and Milwaukee and to London, Oxford, and Berlin. He gives little attention to Soviet collections at the Museum of Anthropology and Ethnography of the Academy of Sciences, Leningrad, and the Anthropological Museum of

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Lomonosov State University, Moscow, which are believed to be among the richest. Nonetheless, we are treated to some remarkable fragments of information about the artists, collectors, buyers, and sellers who joined the scramble for Northwest Coast artifacts. The production of fakes subverted the production of genuine articles, and the activities of nefarious traders undercut the intentions of noble-minded purchasers. Long before state-sponsored collection of native artifacts, white traders such as George Emmons were working a coastal beat with success and profit. Native Americans such as Louis Shotridge also entered the business of selling artifacts to museums, reasoning that relics telling the tale of a vanishing people should be preserved in the interests of history. Anthropologists, archaeologists, and geologists employed by the government and museums not only kept home authorities happy with learned reports and field studies but also, like Marius Barbeau of Ottawa, managed to conduct a considerable business on the side. Collectors for the U.S. Internal Revenue Service unburdened native Americans of their treasures and sold the goods for money. Ottawa's museums benefited from the enforcement of the anti-potlatching law—if police and agents did not take a first share of the artifacts. Even priests proved to be prosperous purveyors. Under these constant demands, the supply of artifacts gradually dwindled, a process aided and abetted by the ravages of disease, population decline, and consequent destruction of native American communities.

Cole's book is an exciting investigation of an important and original topic. It adds considerably to our understanding of the changing roles of native peoples of the Northwest Coast in the late nineteenth century. The photographs, maps, bibliography, notes, and index enhance the quality of this fine work.

BARRY MORTON GOUGH
Wilfrid Laurier University

RICHARD A. BARTLETT. *Yellowstone: A Wilderness Besieged*. Tucson: University of Arizona Press. 1985. Pp. xiv, 437. $24.95.

In this handsome volume Richard A. Bartlett brings to completion his monumental effort of many years, a comprehensive history of Yellowstone National Park from prehistoric times to the present. *Nature's Yellowstone* (1974) brought the narrative up to the creation of this, the first national park, in 1872. *Yellowstone: A Wilderness Besieged* carries the story up to the present.

Rather than presenting a standard chronological narrative, Bartlett takes a thematic approach. The first chapters sketch out the early decades of the park's history, describing the first pioneer tourists, the era when railroad travel predominated, and the coming of car-borne tourism seventy years ago. The author then refocuses intensively on the period between 1872 and 1940, analyzing in depth the superintendents and their regimes (including the long era of U.S. Army rule from 1886 to 1918) both before and after the creation of the National Park Service in 1916, the powerful "concessionaires" who contracted for hotels and other park franchises, the role of the railroads, the struggles against poachers and reclamationist raids, and the development of wildlife management policies. The long period after 1940 is treated in a mere 36 out of 398 pages.

This is certainly not a romanticized celebration of the triumph of democracy in creating our first national park and in maintaining it as a people's pristine playground. Rather, it is a solidly based study of institutions, politics, and entrepreneurship. There definitely are hereoes in the story of the park's preservation—most notably, Yellowstone's self-appointed public defender, Senator George Vest of Missouri—but many more men can only be classed as villains, especially the concessionaires, such as Eli Waters, who used their political influence in Washington to garner and maintain lucrative park contracts that undermined the public interest. Harry Child, the greatest of the concessionaires, made a substantial fortune from his park contracts, and, as recently as the 1960s and 1970s, the conglomerate concessionaire General Host took out millions in profits while failing to make adequate reinvestments, which resulted in deteriorating park facilities.

Bartlett's book has certain shortcomings, including an occasional failure to consult regional journals and an occasional error of fact. The Milwaukee Railroad, for instance, never entered the tortuous Gallatin River Canyon. The most disappointing aspect of the book is its brief treatment of the period since 1940, which constitutes, after all, over one-third of the park's entire lifetime. Bartlett reasons that we lack adequate perspective to deal properly with this modern era, but one wishes for more attention to it, especially since these are among the book's most revealing pages.

In all, however, this is an amply rewarding book. It is thoroughly researched in primary sources, gracefully written, and well conceptualized, and it places the subject in clear regional and national perspective. One learns here, again and again, that even in this fairest of wilderness retreats the ugly issues of politics and profiteering constantly manifested themselves. Scholars and lay readers alike will learn from the pages of this volume, which, along with its predecessor, constitutes the best history of Yellowstone.

MICHAEL P. MALONE
Montana State University

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PAUL E. PETERSON. *The Politics of School Reform, 1870–1940*. Chicago: University of Chicago Press. 1985. Pp. x, 241. Cloth $25.00, paper $11.95.

This is a provocative, theoretically informed, and largely effective study of educational reform in three cities—Chicago, Atlanta, and San Francisco—from the end of the Civil War through 1940. Part 1 examines the politics of education in postbellum America: the widespread support for the common school system among most major groups, including organized labor; the impetus to curriculum reform that "occurred in response to demands from different groups that had the political power to force such an expansion or that had the resources to set up competing systems unless their demands were met" (p. 70); the relatively equitable disposition of school resources as measured by class size and teacher ethnicity across ethnic groups; and the denial of equal access to education of minorities in America, especially blacks in Atlanta and Oriental groups in San Fransisco, as a result of their limited political bargaining power. Part 2 examines the politics of educational reform and the transformation of school governance during the Progressive era through the end of the 1930s and the differing fortunes of public education in the three cities during the 1930s.

Paul E. Peterson develops four main arguments. First, he argues that the primary motive and engine of educational expansion and reform was not the desire of elites to develop and employ schools as institutions of social control or "cultural imperialism." Rather, drawing on Max Weber's distinction between class, status, and power, he suggests that the social sources of educational reform are to be found in competition between social groups for social status or market advantage and the efforts of school administrators to advance the fortunes of public schooling. Moreover, contrary to some revisionist accounts, the American "working class," or at least organized labor, consistently supported educational expansion and reform. Second, he characterizes the politics of educational reform less in terms of a politics of class conflict between business elites and the working class than in terms of a politics of pluralist conflict and competition between shifting coalitions of status groups. Third, Peterson stresses the importance of political power as the means of translating social aspiration into educational policy. Groups integrated into the political system usually succeeded, at least partially, in securing education or school reform; groups excluded from the political system—blacks in Atlanta and the Chinese in San Francisco, for example—were consistently unsuccessful in obtaining educational facilities until such time as they managed to become effective participants in the political process. Finally, he emphasizes that, as a consequence of the pluralist and democratic character of American politics, no one group ever succeeded in capturing control of the public schools and that schools therefore acquired considerable institutional independence and legitmacy.

Against the more vulgar form of social control and class conflict arguments Peterson's book is a remarkably successful effort: intelligent, thoughtful, provocative, and free of hysterical polemics. But not all revisionist histories of education are as reductionist or as simple-minded as Peterson would have us believe. Indeed, Peterson's book begs a great many important conceptual and theoretical questions raised by more recent revisionist accounts that stress the relationship between school reform, complex processes of class formation, and the integration of the school system into the market economy. Peterson fails, for example, to examine the relationship between changes in school attendance, the family economy, and processes of class formation. He arbitarily reduces class politics to class-conscious class-conflict politics. He fails to distinguish between organized labor and the "working class." He fails to provide a systematic account of the varieties of Progressive education and their relationship to each other and to Progressive reform generally. Although he consistently links school politics to local politics, he neglects to link school politics to larger economic, political, ideological, and social developments. And he ignores the development of stratified credentialling markets in the late nineteenth and early twentieth centuries that radically transformed the American stratification system and the role of education in it.

In sum, even though this is an inadequately conceptualized and contextualized work, it nevertheless manages to refute simpler forms of revisionism. *The Politics of School Reform* will not convince every reader that revisionist history of education is intellectually bankrupt, but Peterson's aggressive use of a Weberian and pluralist approach will do much to keep revisionists intellectually honest and in the archives.

DAVID HOGAN
University of Pennsylvania

JAMES G. SMART. *Striving: Keene State College, 1909–1984; The History of a Small Public Institution*. Canaan, N.H.: Phoenix. 1984. Pp. xxviii, 350.

Striving is an appropriate title for this first history of the younger of New Hampshire's two state colleges. James G. Smart, a historian at the college since 1966, focuses on how Keene has evolved from a normal school to a teachers' college to a state college and, more significantly, on how Keene and the teachers' college movement in general, competing with the

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state universities, have struggled to gain adequate respect and financial support. Furthermore, Smart argues that the historiography of higher education, no less than the general citizenry and the state legislatures, share in the neglect of these institutions. One can hardly question the validity of the latter point, but it would be more completely correct to note that nonelite institutions, both public and private, have suffered from this lack of attention and support. Thus, in presenting the history of one of the "colleges of forgotten Americans," as E. Alden Dunham describes the state colleges in his 1968 Carnegie Commission study, Smart has made his own contribution to the greater visibility of such institutions.

If external striving for recognition is one feature of this history, the internal strife between segments of the college is another. More than most institutional historians, Smart discusses the philosophical differences between the professional faculty (at Keene this has meant primarily education) and the liberal arts faculties, the student protests, and the struggles between the presidents and the faculty.

In addition to its conflict motive, other noteworthy aspects of this history include the author's impressive use of oral history (for example, how many college historians can draw on recordings of the earliest students giving detailed evaluations of the classroom performances of the original instructors?) and his citation of what he believes to be Keene's chief claims to national recognition. The latter include, first, three of its campus buildings are listed on the National Register of Historic Places and, second, its founder, Henry Clinton Morrison, was a pioneer in the development of the junior high school and the idea that aspiring teachers should combine theory and practical experience throughout the entirety of their training. In sum, Keene State now possesses a written history worthy of its first seventy-five years.

WILLIAM C. RINGENBERG
Taylor University

A. THACKRAY *et al. Chemistry in America, 1876–1976: Historical Indicators.* (Chemists and Chemistry.) Boston: D. Reidel. 1985. Pp. xxiii, 564.

In 1975 when A. Thackray, J. L. Sturchio, P. T. Carroll, and R. F. Bud offered two seminars at the University of Pennsylvania on the history of modern chemistry, they immediately faced one serious handicap: the "lack of reliable quantitative knowledge about past science" (p. xiii). That experience led to the present volume, a vast compilation of long-run trends—or, as the authors call them, "chemical indicators" (p. 2)—in the history of American chemistry. Their hope is that this work will provide the foundation for a full-blown cultural history of the institution of chemistry in this country.

The book is a historical statistician's dream. Largely a reference tool but part monograph as well, *Chemistry in America* contains 89 pages of graphs, 11 pages of text tables, 251 pages of appendix tables, and 104 pages of narrative. The volume's findings (its chemical indicators) are succinctly summarized at the start of each section, allowing easy access for readers who want only to sample the book's conclusions.

Every facet of the institution of chemistry gets attention. Separate chapters deal with chemistry as occupation and profession; the education of chemists; the growth of the chemical industry; employment in industry, academe, and government; and chemistry as a discipline, a chapter that focuses on the historic role of chemists in the American research university and on the mounting international reputation of their work.

To some degree the trends that are identified merely quantify (and occasionally belabor) the obvious. The reader learns that the number of chemistry degrees has increased exponentially and that the American chemical industry "has grown dramatically . . . in absolute and relative terms" (p. 85). Yet there are interesting and unexpected findings, too. One trend that the authors identify is a "progressive decoupling" of chemistry (p. 39), meaning that those educated in chemistry have increasingly entered some other employment than the chemistry profession or occupation (for example, the chemistry undergraduate who goes to medical school). Also surprising is the finding that since 1950 chemists account for a greater proportion of working scientists (25 to 33 percent) than of members (13 percent) of the National Academy of Sciences (NAS). Such surprises are plentiful enough to keep readers interested.

One disappointment is the authors' general unwillingness to hazard guesses as to why certain trends occurred. This reader would have liked an explanation of the poor showing of chemists in the NAS, especially in view of the rising dominance of American chemistry in the world. And it would have been useful to have an insight into why chemists (since 1950) have formed an ever-smaller part of the nation's professional, technical, and kindred workers. But, considering the vast amount of work that went into this eminently useful volume, it is probably unreasonable to expect a discussion of causation, too. Besides, it is those sorts of inquiries that *Chemistry in America* aims to—and will—stimulate and assist.

E. H. BEARDSLEY
University of South Carolina

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PHILIP B. SCRANTON, editor. *Silk City: Studies on the Paterson Silk Industry, 1860–1940*. Newark: New Jersey Historical Society. 1985. Pp. 176.

Although most collected essays offer a hodgepodge of views and styles, the essays that Philip B. Scranton has brought together form a surprisingly coherent view of the development of the silk industry in Paterson, New Jersey, and the impact of that industry on the social relations of the city.

Located next to New York City, the nation's major distribution center for nondurable goods, Paterson's silk industry grew under the shadow of the city's machine-building and cotton-textile manufacturing. Nearness to New York brought Paterson immigrants with silk-making skills, and the Passaic River brought the city power and water. Closeness to the volatile New York City fashion market transformed Paterson into "silk city." Yet within that history of growth were buried the seeds of future decline.

As Richard Margrave's essay aptly demonstrates, Paterson's silk industry depended on highly skilled labor, most of it immigrant labor that came first from England and France and then from northern Italy and Poland. These skilled immigrants possessed a strong sense of themselves as skilled craftsmen. They also developed a militancy and tenacity regarding a method of work that challenged the authority of the manufacturers caught within a tight credit and marketing discipline. The conflicts that pitted manufacturers against their workers and against their creditors powered the forces behind the transformation of Paterson's production system.

Scranton's essay places the production of silk textiles within the structure of the larger political economy—the financing, credit, and marketing system of the silk trade. Silk was the material of high fashion, and its market was highly volatile. This put pressure on manufacturers to control costs by reducing wages, increasing production, and decreasing the quality of the goods. Since the first two options generated massive worker resistance, employers adapted strategies to subvert that resistance. Those strategies and the reduction of quality ultimately took their toll on Paterson.

Steve Golin finds that a combination of factors contributed to the manufacturers' victory over workers in the 1913 strike: the domination of large manufacturers, with subsidiary production sites outside the city; worker refusal to accept concessions from smaller firms without a citywide agreement; and manufacturers' belief that only through unity could they control the production process. Ultimately, the strike was defeated by the production sites outside of Paterson. Manufacturers originally developed these sites because they respected the militancy and tenacity of Paterson's silk weavers, which often challenged their authority.

Patricia O'Donnell looks at the operation of business from the perspective of the product itself. Although she focuses on the patterns and designs of silk fabrics, her essay is instructive on the nature of the highly fashion-conscious silk market.

David Goldberg looks at labor's organizational and strategic response to the manufacturers' repeated attempts to control labor costs. Goldberg analyzes how ethnic and sectoral divisions within the working class provided the instruments of resistance and resilience in face of defeat, as well as the cause of the factionalism that hindered the building of a classwide union.

Philip McLewis investigates how manufacturers reverted to a structure of production that replicated the putting-out system in order to maximize labor productivity while minimizing resistance to stretch outs. Workers, if they wanted to continue as weavers, were forced to buy their machines, rent space in subdivided old mills, and employ the labor of the family. The old manufacturers supplied the raw silk, extended credit, and bought back the finished product. Under this system workers became "sweaters," sweating their own families. With many high fixed costs and few variable ones, these worker-owners maximized output, which led to longer hours and more machines per capita. As might be expected, these shops were not stepping stones to entrepreneurial success but slippery rocks to financial failure.

Scranton has brought together an excellent collection of essays. It deserves a wider audience than, I suspect, it will get.

JOHN T. CUMBLER
University of Louisville

ROBERT W. GARNET. *The Telephone Enterprise: The Evolution of the Bell System's Horizontal Structure, 1876–1909*. (Johns Hopkins/AT&T Series in Telephone History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xxiii, 210. $22.50.

GEORGE DAVID SMITH. *The Anatomy of a Business Strategy: Bell, Western Electric, and the Origins of the American Telephone Industry*. (Johns Hopkins/AT&T Series in Telephone History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xxii, 237. $20.00.

The American Telephone and Telegraph Company (AT&T) recently opened its rich collection of historical documents to scholars. To publicize its decision and to demonstrate its commitment to serious scholarship, the company is sponsoring a series of books on the history of the telephone industry. The two studies under review, both commissioned by AT&T, are the first titles in this series. Written by professional historians and intended as contributions to

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the literature on large-scale, managerial enterprises, they are both solid works of scholarship. My only criticism of the project thus far is that the studies are based somewhat too narrowly on the holdings of the AT&T archives.

Both authors endeavor to explain how a small, weakly capitalized entrepreneurial venture transformed itself into the wealthy bureaucratic giant we are familiar with today. George David Smith focuses primarily on the process of backward integration, in particular the firm's acquisition of control over the Western Electric Company in 1882. Robert W. Garnet concentrates on the geographical expansion of the company—its gradual increase in control over local telephone exchanges—and the concomitant elaboration of a managerial hierarchy. Despite this division of labor, the two books overlap to some extent, but the authors are not always in complete agreement. For example, according to Garnet, the rivalry that developed between Bell Telephone and Western Union in the late 1870s was not characterized by price competition (Garnet, p. 32). In contrast, Smith argues that Western Union did in fact "cut prices (and, on occasion, Bell lines)" (Smith, p. 38).

The two authors also differ on larger matters of interpretation, although both agree that efficiencies resulting from the combination of mass distribution and mass production (Alfred D. Chandler, Jr.'s theme) cannot account for the telephone company's initial growth and success. Smith suggests that the most important motive for vertical integration was the firm's need to protect itself from future competition. The telephone company acquired Western Electric for its patents as much as for its productive capacity; in fact, Bell officials considered Western Electric to be neither a high-quality nor an efficient producer. Similarly, forward integration, in Smith's view, was a means of preventing local exchanges from being lured into the networks of rival companies.

Garnet acknowledges the importance of the market-control motive but insists that there were other significant reasons for vertical and horizontal combination. For example, Bell was driven to acquire equity in local operating companies by the exchanges' own demands for capital as well as by a technological imperative (deriving from long-distance telephony) to standarize equipment. Equally important were the system-building visions of some of the company's top officers, particularly Theodore N. Vail, a former superintendent of the Railway Mail Service, who was installed as president by New England financiers.

Despite their differences, both authors underscore the tenuous nature of the company's early development. They vividly describe the weaknesses of the original venture and portray the enterprise's subsequent success as the product of trial, error, and a not inconsiderable amount of luck. Although there is undoubtedly some truth in this, a skeptical reader will note the rapidity with which the firm established its position of leadership within the industry. In just six years following Alexander Graham Bell's invention of the telephone in 1876, the firm increased its capital from a couple hundred thousand dollars to ten million, secured a promise from the giant Western Union Company not to enter the telephone market, acquired a controlling interest in Western Electric and other manufacturing firms, and purchased equity in local exchanges throughout the country. This rapid transformation resulted primarily from the backing of New England financiers, whose successive interventions brought the firm not only infusions of badly needed capital but also, as Garnet's analysis shows, experienced managers skilled in building and operating a national system. Although not the authors' intent, one can read into their works the framework for another alternative to Chandler's technological explanation of the rise of big business—one that highlights the organizational culture in which these northeastern financiers were immersed.

NAOMI R. LAMOREAUX
Brown University

LEONARD S. REICH. *The Making of American Industrial Research: Science and Business at GE and Bell, 1876–1926*. (Studies in Economic History and Policy, The United States in the Twentieth Century.) New York: Cambridge University Press. 1985. Pp. xvi, 309. $24.95.

Leonard S. Reich begins with a hope that his book will reach managers and industrial researchers as well as scholars. It should, because it is packed with insight into the process of organizing and managing corporate research. Harnessing scientific research to corporate goals has always been fraught with problems of balance, and the resulting tensions come alive in Reich's studies of General Electric (GE) and American Telephone and Telegraph (AT&T). From the turn of the century to the mid-1920s, these two companies pioneered in creating formal research organizations that served strategic and corporate goals while maintaining the air and, to a large extent, the reality of autonomy from the immediate demands of production. Structures were important, but so were people. Industrial laboratories could not succeed without resources and environments sufficient to attract some of the best minds working in science and technology, nor could they succeed without top management advocacy or skillful research directors. Many of the lessons learned by GE and AT&T are generic and, as such, are eminently transferable.

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The book contains riches for the professional scholar also. Reich has a good command of the historiography of both business and technology, and his own work fits nicely into what Louis Galambos has identified as the emerging "organizational synthesis" of modern American history. Reich accounts for the cultural values and the educational and professional developments that supported the marriage of science to technology and then both to business ends. From its origin in the engineering laboratories and patent departments of high-technology companies, he traces the rise of the corporate research laboratory as it evolved into the dominant force in the generation of new technologies. All told, he provides us with an admirable synthesis of historical wisdom on industrial research that far transcends his particular cases.

The cases comprise the main body of the book and compare GE's and AT&T's responses to the constraints posed by their technologies and managements as they developed their research functions. Both companies were stable organizations, dominant in their industries. They established formal research organizations in reaction to competitive threats that could be countered only through control of deeper, more systematic scientific knowledge. Until GE's laboratory had demonstrated a capacity for major breakthroughs, it struggled for support from its corporate management; only then could it blossom into a font of new products. Because GE's products were varied, its research evolved along diverse lines and methods under conditions of relative freedom. Formal research at AT&T was promoted by top management and quickly became an integral part of the company's long-distance network development. Telephone technology was systemic and its markets uniform, and so AT&T's research became, by comparison to GE's, more highly focused, more tightly structured, and more concerned with incremental improvements. Dramatic breakthroughs were achieved but on a narrower front. Turning to the struggle of both companies to control radio, Reich assesses the impact of sophisticated industrial science on corporate strategies for the penetration of new markets and the defense of old ones.

Reich's analysis of the laboratories, research activities, and key personnel draws heavily on archival records, but for some reason he relies instead on the FCC's adversarial report, *Investigation of the Telephone Industry* (1939), for his documentation of AT&T's strategy for radio. In accepting the FCC's indictment of AT&T's radio strategy as a quest for monopoly in a new field, he misses an opportunity to make a relevant point. Relying solely on the evidence cited, we can see that AT&T, given its "business-technology mind-set," probably did not view radio as a business distinct from telephony. Elsewhere he gives short shrift to the creation of Bell Laboratories in 1925. Just what drove this radical restructuring of research activities (by severing research from manufacturing and reconstituting it as a separate subsidiary) goes unstated.

Future histories of the corporate laboratory would do well to follow Reich's lead in accounting for the myriad social, technological, economic, institutional, and human factors that have determined the course of industrial research. Beyond that lies the largely uncharted path of innovation: the course along which the discoveries of research have been refined through development, engineered into production, and sold in the marketplace.

GEORGE DAVID SMITH
Winthrop Group, Inc.

JOHN N. SCHACHT. *The Making of Telephone Unionism, 1920–1947*. New Brunswick, N.J.: Rutgers University Press. 1985. Pp. x, 282. $30.00.

The evolution of the telephone workers' union followed a different course from that of most American labor organizations. In this scholarly study John N. Schacht traces the history of telephone workers' organizing efforts from the company unions of the 1920s to the founding of the Communications Workers of America in 1947.

When American Telephone and Telegraph (AT&T) integrated regional and state telephone companies, the long lines' network, and Western Electric's equipment manufacturing plants into one large corporation, it created an organization that spanned the continent and reached into most homes and businesses. Although AT&T announced that each unit was responsible for its wage and labor policies, they were, in fact, under central control. Available direct evidence is scanty, but the pattern of common policies and settlements affords ample ground to infer AT&T's control.

The Bell system's work force defied easy classification. The installers of equipment were skilled tradesmen, operators were only semiskilled, and Western Electric factory employees were mostly blue-collar machine operatives. The skilled tradesmen and factory workers were male, with a normal age distribution, but the operators were overwhelmingly female, youthful, and, many of them hoped, only temporarily workers. Although not impossible to organize, they lacked the common experiences, background, and focus that readily bind workers together.

From 1920 to 1935 Bell fostered company unions as a channel for grievances. When the Wagner Act declared company unions illegal, Bell substituted new organizations that were nominally independent but mostly company-dominated. Without assistance from the American Federation of Labor or the

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Congress of Industrial Organizations (CIO), Bell employees moved slowly toward genuine unions. In 1939 many locals allied in the National Federation of Telephone Workers, but "autonomania" rendered that organization ineffectual.

Interest in a new union grew as World War II brought deteriorating working conditions and, in contrast to the experience of workers in other industries, lower real wages. A few brief strikes revealed workers' militant mood and taught them the costs of local autonomy. In 1946 a nationwide agreement brought AT&T "into the open" as the directing force in labor relations (p. 152). Confident of its strength, the corporation forced a strike in 1947 of three hundred thousand workers. It was the first nationwide telephone strike, the largest strike of that year, one that reached into more communities than any other in American history, and, because nearly two-thirds of the strikers were women, the biggest walkout in the history of American women. Although the strikers drifted back to work, the movement toward a national union was not delayed. In June, workers founded the Communications Workers of America, an industrial as well as a national organization that quickly affiliated with the CIO.

The major theme of this work is that the movement for a centralized and bureaucratized union emerged as the only rational, potentially effective response to the labor policy of a centralized, bureaucratized, and powerful corporation. Schacht has found no evidence that centralization in a single national union resulted from the ambitions of leaders who were eager to manage or suppress the discontent below while creating pleasant and prestigious positions for themselves. Since some labor history scholarship portrays the process of union formation and the dynamics of bureaucratization as the result of union leaders' ambitions, Schacht's thoroughly researched, carefully balanced, and persuasive discussion of the telephone workers' union deserves attention and acceptance.

JOHN BARNARD
Oakland University

CHRISTOPHER L. TOMLINS. *The State and the Unions: Labor Relations, Law, and the Organized Labor Movement in America, 1880–1960.* (Studies in Economic History and Policy: The United States in the Twentieth Century.) New York: Cambridge University Press. 1985. Pp. xvi, 348. Cloth $39.50, paper $12.95.

This is an institutional history of both unions and the legal regulation of labor between 1880 and 1960. The book's initial section deals with the period between 1880 and the New Deal, stressing the transformation of America's political economy and the concurrent judicial restrictions on union activity. Nineteenth-century legal regulation is analyzed as part of an ideological battle over the true meaning of republican values. This section, indeed the book as a whole, takes ideology seriously and views law as an embodiment of ideological notions.

Unions, formed to avoid the problems of individualism and individual competition, were indeed in conflict with the prevailing legal values. Christopher L. Tomlins stresses that unions saw their legitimacy not simply in light of whatever legal rights might be granted but rather as republican institutions in their own right. Courts, therefore, were viewed as a threat to republican institutions with which unions identified. The almost exclusive stress on the American Federation of Labor (AFL), however, excludes other attempts to respond to the economic system, such as the Knights of Labor or the Industrial Workers of the World.

The heart of the book deals with the creation of federal regulatory labor policy in the 1930s and early 1940s. The critical focus is on the rise of industrial "pluralism," a set of doctrines that recognized conflicts of interest in the workplace but presented these disputes as being susceptible to adjustment rather than being inherent in the nature of the employment relationship itself. Collective bargaining was seen as one of the mechanisms for "managing" industrial conflict, and the collective agreement was the primary social goal. Tomlins stresses the cost to unions both of the Wagner Act of 1935 and, especially, of embedding pluralistic thought into the statutory scheme.

Tomlins carefully delineates the extent to which the Wagner Act, and at least its initial regulatory focus, altered principles embodied in AFL ideology. The AFL, for instance, believed that a union's claim to status rested not on any formal designation as a representative of particular employees but on the union's institutional position within the labor movement as the rightful claimant to jurisdiction over certain workers. Under the statute, however, a union's role was to serve as an "agent" of employees, carrying out a public role, and not as owner of jurisdictional "property rights" developed within the union movement. Thus, Tomlins argues, the rise of the Congress of Industrial Organizations, as well as its close relationship to the National Labor Relations Board, was as much the "occasion" for confrontation betweeen the AFL and the New Deal as the cause. The argument over the appropriate contours of bargaining units and the agency's policy toward them, therefore, is seen as emblematic of an ideological battle and not simply as a cover for self-interested concerns.

After 1939 the board, because of changes in membership, began to weaken its commitment to

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the "centrality of public ordering in policy formulation," permitting greater deference to the actions of the organized parties (p. 198). The argument is predominantly based on the writings of board members and their legal decisions. The growing desire to protect established collective bargaining relationships and union incumbents, heightened during World War II, even came to outweigh the policies of self-determination of employees. Concomitantly, the interest in stability led to a narrowing of the scope of allowable employee action that might violate the contract or that might otherwise upset institutional bargaining arrangements. The direction, then, was toward stability and regularity and away from industrial democracy.

Tomlins argues that legal interpretations of the act, combined with the acquiescence by major unions, would eventually entrench pluralist ideology into the statute and tightly lock unions within the structure. His conclusion is that union gains under the statute have amounted only to a "counterfeit liberty," because the current legal accommodation offers workers "ultimately no more than the opportunity to participate in the construction of their own domination" (p. 327).

The focus on ideology has value but tends to avoid the possible impact and relevance of nonlegal events. The war, for instance, plays only a minor role in the book, perhaps because Tomlins believes that the shape of postwar legal rules was inherent in prewar developments. Tomlins does not adequately explain why unions acquiesced in harmful governmental policies or why the views of the leadership of the AFL merit exclusive attention. As an institutional history, finally, little mention is given to workers themselves or the extent to which legal policies and rules actually affect the behavior of the parties. Nevertheless, this book is a valuable contribution to the emerging literature revising traditional perceptions of labor regulation.

JAMES B. ATLESON
State University of New York, Buffalo

DONALD E. WINTERS, JR. *The Soul of the Wobblies: The I.W.W., Religion, and American Culture in the Progressive Era, 1905–1917*. (Contributions in American Studies, number 81.) Westport, Conn.: Greenwood. 1985. Pp. 159. $27.95.

This little book is basically a variation on the theme that Herbert Gutman originally enunciated in his essay "Protestantism and the American Labor Movement" (1966). Through an examination of the newspapers, songs, and poetry of the Industrial Workers of the World (IWW), Donald E. Winters, Jr., seeks to prove that a religious imagery and symbolism drawn from the Protestant New Testament inspired the Wobblies: "The I.W.W. is a valuable lesson about how religious sensibility and imagery helped mobilize the radical element of the labor movement in a way that guarantees the Wobblies a permanent and important place in American labor lore" (p. 131).

Winters, however, unnecessarily weakens his attempt to establish the salience of religion for the history of the IWW. First, he defines religion too elastically, as "a system of beliefs and symbols which seeks to develop in the working class a sense of solidarity and class consciousness, and a motivation to engage in a class struggle against the evil force of capitalism toward the end of creating a new order . . . in the shell of the old" (p. 11). Such a definition encompasses every modern radical workers' movement and also many more conventional trade unions, which seek to inculcate solidarity and class consciousness among their members as a basis for class struggle. Second, Winters confines his analysis solely to Protestant imagery in the early twentieth century, when a majority of American workers were non-Protestant. What we learn from this extended essay is how a small number of Wobbly leaders and writers—most notably, Covington Hall, Ralph Chaplin, Joe Hill, and Eugene Debs—used the Protestant symbols with which they were familiar to rationalize secular forms of radicalism. How such religious imagery was assimilated and used by its audience remains an unaddressed and unanswered question. Surely, the Catholic immigrant workers of Lawrence, Paterson, Akron, Detroit, and the mining and mill towns of the West felt more comfortable with other religious traditions and symbols. And what of Jewish workers for whom allusions to Jesus' martyrdom as a revolutionary might smack of latent anti-Semitism?

On the larger questions of the IWW's place in American history and the significance of radicalism for workers in the early twentieth century, Winters remains largely inattentive. Implicitly, however, his research and findings dispute Aileen Kraditor's assertion that American radicals were aliens in their own society, men and women who spoke a language at odds with that of most ordinary workers. Moreover, the subject of religion aside, Winters tells us little that is new about the IWW and its history. Indeed, his general grasp of the specifics of labor and radical history scarcely matches his explication of the religious bases of the IWW's ideology.

Perhaps the best way to close this review and capture the spirit of the book is to cite the words of a Wobbly expressed on the penultimate page: "The modern militant of the labor movement may without hypocrisy pause to do homage to the humble yet heroic carpenter of Nazareth. Despised as we are despised; hunted as we are hunted—he seems like

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one of our kind, with whom we may clasp fraternal hands across the centuries . . . since his ideal of universal brotherhood based upon toil is not forgotten—and is about to be realized."

MELVYN DUBOFSKY
State University of New York, Binghamton

TERRY P. WILSON. *The Underground Reservation: Osage Oil.* Lincoln: University of Nebraska Press. 1985. Pp. xiv, 263.

This is the first complete and reliable account of the origins and dynamics of leasing Osage tribal lands for oil exploration as well as of the better-known sociological effects of the enormous wealth that royalties from oil production brought the Indian tribe. Since the first direct treatment of the oil issue comes on page 99, it is tempting to say that this book has the wrong title. As Terry P. Wilson develops his narrative and argues convincingly that the oil story cannot be understood except as a logical outgrowth of tribal changes and prior leasing experiences, that criticism is reduced to a suggestion that he might have given the reader earlier hints of where he was leading. But, because Wilson places the mineral issue in such a deep sociological context, one has to rate his study among the most satisfactory Osage tribal histories for the post-1865 period.

The Osage oil story has fascinated popular writers, but few serious historians have analyzed the topic. Like all first-rate historical contributions, this book has been carefully and painstakingly researched in primary sources. Anyone who has seen the nearly one hundred boxes in the National Archives marked "Osage leases" or considered the linear yards (maybe miles) of Osage records located at the Federal Records Center in Fort Worth, Texas, has special respect for someone who would tackle this subject. The result of Wilson's effort is a multifaceted scenario that is much more complex than the standard view of tribal reality and is therefore not suited to casual skimming. It does, however, reward study and should serve as a standard for the study of intercultural contact in the industrial age.

Few chapters in this book are without surprises, even for the well-informed. The extent and duration of intratribal rivalry among the Osages and its effect on oil leasing and royalty policy are detailed as never before. Wilson traces the impact of whiskey and other drugs on a case-by-case basis from the 1880s to recent times and follows clearly and accurately internal politics, from difficulties with the police force to guardianship frauds. He writes fine social history describing lease auctions, spending sprees, and famous incidents of violence, intermarriage schemes, "visiting" of the rich Osages by more impecunious tribes, and council debates. Finally, Wilson's detailed presentation of the complexities of trust-fund manipulation shows ability to use statistics without sacrificing clarity or interest. Perhaps the most useful feature of the text is its linking of the nineteenth with the twentieth century and its fine historical explanation of the state of the tribe since 1940.

This is a book that will endure and will not be left behind either by the season or by increasingly sophisticated views of Indian history. Wilson was inspired—nearly overawed—by John Joseph Mathews's seamless web of Osage history but noticed that the work was weak where the traditional culture touched on the changes that occurred in Mathews's own lifetime. That is where Wilson took the story up. Although a different type of book than Mathews would have written, *The Underground Reservation* is every bit the equal of its model, if not in style, then in its contribution to research on the Osage.

CRAIG MINER
Wichita State University

JOHN HOPE FRANKLIN. *George Washington Williams: A Biography.* Chicago: University of Chicago Press. 1985. Pp. xxiv, 348. $24.95.

In this model biography John Hope Franklin, the dean of Afro-American historians, has produced a definitive study of George Washington Williams, pioneer historian of black history who was also a maker of history. Franklin's attention was caught back in 1945 by Williams's two-volume *History of the Negro Race in America* (1882). This attraction resulted in a paper on Williams published in *The Journal of Negro History* in January 1946. Ever since, Williams has never been out of his biographer's mind; decades of research in the United States, Europe, and Africa have been distilled in this volume.

Although Williams (1849–91) did not reach his forty-second birthday, he managed to pursue half a dozen careers, several with distinction. In his fourteenth year he lied his way into Lincoln's army and saw a year's combat, suffering a wound. After Lee's surrender he volunteered for service with the liberating forces of Benito Juarez and rose to the rank of lieutenant. He followed this with a year's service in the famous Tenth Cavalry, U.S. Army, from which an accidental gunshot wound hastened his discharge.

Williams enrolled at Howard University and Wayland Seminary and overcame illiteracy, gained entrance to Newton Theological Institution and graduated in 1874, and then served as a Baptist minister in two Massachusetts churches. After briefly editing his own newspaper, *The Commoner*, in

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Washington, D.C., he moved to Cincinnati in 1876. Here he impressed Murat Halstead and served as a columnist for Halstead's Cincinnati *Commercial* for almost two years (1876–78), meanwhile continuing his services as a Baptist pastor. He also gained distinction in Ohio politics, studied law under the famous Alphonso Taft, and from 1880 to 1881 served in the Ohio House—the first black to hold such office in that state. Franklin details Williams's meaningful activity as legislator.

Williams was admitted to the bar in both Ohio and Massachusetts but, meanwhile, had decided to write a history of black people. His research was thorough, and, insofar as he interviewed many witnesses and diligently searched newspapers as well as more conventional sources, he pioneered not only in subject but also in methodology. He supplemented his well-received two-volume history with the still-useful *History of Negro Troops in the War of the Rebellion* (1887). Williams's outstanding position among Afro-Americans resulted in his appointment, in March 1885, as U.S. minister to Haiti by President Chester A. Arthur, but the new Democratic president, Grover Cleveland, revoked the appointment.

In his last years Williams spent much time abroad in historical research, in some church-related duties, and—having had an audience with Leopold II of Belgium—in a prolonged and exhausting examination of the king's Congo domain. In this connection, Williams reached his finest hour, manifesting selfless courage by publishing in 1890 an exposé of the horrors perpetrated by speculators and exploiters in the Congo (reprinted here as an appendix). Other reports, equally truthful, were prepared for President Benjamin Harrison and for Collis P. Huntington, the railroad tycoon whose interest in railroading in Africa had led to contact with Williams. The African journey, plus the gunshot wound sustained in the Tenth Cavalry, resulted in Williams's death while seeking recovery in Blackpool, England.

Franklin does not hide his esteem for the remarkable Williams, but his book is no hagiography. On the contrary, readers are shown "a flawed but brilliant human being . . . capable of misrepresentation if not downright falsehood" (p. 241), and Franklin states bluntly that Williams made "claims for himself that were patently false" (p. 40). This flaw—and some irresponsibility in terms of personal obligations—do not negate the author's estimate of Williams as a significant and woefully neglected American with "a deep compassion for the dignity as well as the rights of darker peoples everywhere" (p. 240).

HERBERT APTHEKER
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ROBERT A HILL *et al.*, editors. *The Marcus Garvey and Universal Negro Improvement Association Papers*. Volume 1, *1862–August 1919*; volume 2, *27 August 1919–31 August 1920*; volume 3, *September 1920–August 1921*; volume 4, *1 September 1921–2 September 1922*. Berkeley and Los Angeles: University of California Press. 1983–85. Pp. cxii, 579; lv, 710; lvii, 811; lv, 1125. $32.50; $32.50; $38.50; $65.00.

Historical work on the pan-African leader Marcus Garvey and his Universal Negro Improvement Association (UNIA) has been hampered by the absence of personal and organizational papers and the inadequate frameworks employed to interpret the man and the movement. This projected ten-volume series provides us finally with a large published collection of source material. But the success of the venture is incomplete because the editing lacks a clear, consistent approach to Garvey and Garveyism.

The first four volumes in the main series on Garvey's political development and the impact of Garveyism in the United States are the results of painstaking detective work. They include material on Garvey's early life and politics in Jamaica, his trips to England and Central America, and his decision to leave Jamaica for the United States in 1916. They document his association with the militant groups of wartime Harlem and the rapid growth and growing difficulties of the UNIA between 1919 and 1922.

Because Garvey's and the UNIA's papers were seized, lost, and destroyed, the project cannot aspire to completeness. Little personal and organizational correspondence remains; we see the public Garvey and UNIA, presented in the pages of his newspaper and observed by government officials, critics, and associates. The material published is an extensive version of widely scattered documents known to and used by researchers.

Some of the richest parts of the volumes are the annotations on persons and events mentioned in the documents, although it is unclear why men such as Leo Tolstoy, Roscoe Pound, and William Jennings Bryan require extended identification. The capsule biographies of participants in the movement are very valuable. Many blacks, it is evident from the notes, joined or aided the UNIA while simultaneously working with "mainstream" racial organizations. The annotations suggest that the ideological differences between Garvey and the Afro-American leadership were smaller than some existing literature indicates. Unfortunately, the results of the fine research are not integrated into the general introductions or interpreted in the notes.

Annotations are rarely used to clarify issues. Many notes meticulously attempt to verify correct versions of texts, but most of the variations lack historical significance. The textual revisions and the extraneous information in the notes simply parade scholarship; fundamental interpretive problems are

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ignored and political terms are used imprecisely. At the very least, some evaluation should have accompanied the documents from surveillance bodies, material whose reliability varied considerably.

Because the items from diverse sources on different aspects of the movement are presented in chronological order, it is often difficult to follow the train of Garvey's thought or the progress of any one of his projects. A different arrangement or more analytical introductions would have made the material more accessible.

Editor Robert A. Hill's long introduction in the first volume adopts a vague thematic structure, abstracts Garvey from his historical context, and uses ponderous language that obscures the significance of the leader. A clear exposition of Garvey's ideas and practices over time and their place within the politics of the United States, the Caribbean, and Africa would have served as a better guide to the documents.

Hill attributes the rapid growth of the UNIA to Garvey's radicalism, which, the editor believes, was influenced by Garvey's experience in the United States. Thus, Garvey's announcement in October 1921 that black people needed political, not industrial, leaders was a declaration of independence from Booker T. Washington (vol. 1, p. lxvii). Hill adopts a narrow definition of radicalism; he seems to mean voting. In "Retreat from Radicalism" different categories produce a contradictory conclusion. Hill finds that after July 1921, Garvey yielded his "prior revolutionary rhetoric" and "revolutionary posture" (vol. 1, p. lxxviii).

The new radicalism of October is at war with the surrender of July; shifting criteria and a cloud of verbiage obscure the changing methods that Garvey used to mobilize his constituencies. Garvey had moved beyond Booker T. Washington earlier when he used militant rhetoric to organize the Black Star Line, a pan-African steamship company, in 1919. The October 1921 announcement was generated by a different transition, the crisis of the Black Star Line (vol. 4, p. xxxi). The endorsement of politics must be interpreted as a response to the failures of Garvey's previous method, using the economic promise and power of the Black Star Line to build a mass movement. Interpreting the public discourse of a man who practiced a "religion of success" (vol. 1, p. xli) apart from his personal and organizational achievements and setbacks is perilous.

Key ideas in Garvey's nationalism, often at variance with the nationalisms of our day, are treated as problematic. Why is it paradoxical, as Hill thinks, that Garvey "held up to blacks the system of European civilization" as a model (vol. 1, p. li)? Garvey, like other nationalists of that time, dissented from the racial, not the cultural, ideas of Western elites. A discussion of ideology and practice, not themes, could resolve some of the puzzles and help place Garvey in a changing tradition.

By the 1920s, Garvey no longer advocated "civilizing the backward tribes of Africa" and urged the economic development of Africa and the political control of that continent by Africans. The introduction simply explains "as events transpired" the old view was transformed (vol. 1, p. lxxxvii). This important transformation, which went through various intermediate stages, requires an evaluation of the impact of World War I, bolshevism, Wilsonianism, and new mass movements on Garvey's politics.

When Hill addresses questions of influence more deliberately, he does not distinguish between verbal support and replication of specific political methods. Thus, he identifies Garvey's statements in support of Irish nationalism with the adoption of the Irish model of revolutionary change. The documents support the former, but they do not validate the latter.

Because of the uncertain intellectual framework, many significant documents are buried. Volume I includes fascinating reports of UNIA meetings in Jamaica from 1914 to 1916. The issues debated, the relationships between rich and poor, black and white, agriculture and industry, reveal Garvey's development of a world view like the one made famous by Booker T. Washington (and a spirited Jamaican opposition to it). But Hill has concluded that Garvey brought from Jamaica the idea of peasant independence and self-government. Hill is confusing the peasant's quest for autonomy with political independence, and no group in Jamaica at this time was urging independence. These documents do not confirm Hill's (C.L.R.) Jamesian conception, but they provide important insights into Garvey's social thinking on the eve of his departure for the United States.

Questions of interpretation affect the kind of material collected as well as the usefulness of published documents. We learn that morale was high in August 1921, but by October the UNIA was in deep crisis (vol. 4, p. xxxi). There were more divisions and branches in August 1921 than there were the year before. Yet Garvey told the UNIA convention that few of the branches sent in their reports and that many were in turmoil and demoralized (vol. 3, pp. 614–16). Without a firmer sense of what was happening in August, the October crisis is incomprehensible.

But little material here allows us to explore these issues. Few accounts of the activities of UNIA locals, available from reports in the *Negro World*, black newspapers of the period, and the branch files of the National Association for the Advancement of Colored People appear in the volumes. Selecting a sample would have required an analysis of the strengths and weaknesses of popular Garveyism, an

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important topic ignored in the introduction. By reprinting division reports mainly from government agents, the editors permit the government to define the issue.

The volumes include the words of many of Garvey's critics. In 1921, his major critic was Cyril Briggs, who questioned the UNIA idea of building a black political movement on the base of economic enterprise. The editors include Briggs's personal and rhetorical attacks but not his substantive criticisms. Hill trivializes the differences between the two men when he asserts that Briggs created the African Blood Brotherhood simply "to combat the spread of Garvey's influence" (vol. 1, p. 523). By viewing Briggs, and also W. E. B. Du Bois, as mere competitors to Garvey, the editors minimize the issues confronting and dividing Garvey and other black leaders of the period. They unwittingly emphasize a petty contentiousness that undercuts their effort to provide material for a serious reexamination of Garveyism.

Hill, the general editor, and Emory Tolbert, the senior editor, their staff at UCLA, and the many historians and archivists who tracked down the documents and excellent photographs have lightened the task of historians. The sheer weight of the collection, however, should not discourage us from asking questions that will bring to light other material relevant to scholarship on Garvey and Garveyism.

JUDITH STEIN
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J. PETER BURKHOLDER. *Charles Ives: The Ideas behind the Music*. New Haven: Yale University Press. 1985. Pp. xiv, 166. $17.95.

Charles Ives (1874–1954) was an American composer who wrote his most significant compositions during the Progressive era but who lived at that time as a genteel businessman isolated from the musical world. Only after he stopped composing in the 1920s did he gradually achieve musical recognition, and since his death he has come to be acknowledged as America's preeminent composer of art music. For the student of American history, Ives's importance lies in the path-breaking experimentalism of his eclectic works; in his use of literary and philosophical "programs," often of an "American" nature, in his compositions and his quotation of American tunes; and in the social context of his career as a composer, particularly the problem of his musical isolation.

J. Peter Burkholder offers us an important study of "the ideas behind the music" of Ives and promises a more technical companion volume, *The Evolution of Charles Ives's Music*. The book in hand, however, is nontechnical and traces the evolution of Ives's "aesthetic views and artistic aims." The mature Ives aimed to represent human experience—particularly his personal experience—in music and "to create a distinctively American music." Behind these aims lay his dualism (his tendency to oppose "substance" and "manner" in art, for example); his belief in idealism, in both art and life; and his emphasis on self-reliance (as in his own musical experiments). Ives summed up his mature artistic thought in his *Essays before a Sonata* (1920), a book whose transcendentalism has led many to assume that Ives had been inspired by Emersonian transcendentalism from the beginning of his composing career. Burkholder devotes much space to correcting this view. He offers a useful periodization of Ives's artistic life and attempts to chart the tortuous course of Ives's aesthetic views, placing his chief emphasis on those figures who successively influenced Ives. The most important of them were Ives's father, a "town musician" and experimenter in sounds in Danbury, Connecticut; his teacher at Yale, the noted composer Horatio Parker, who imbued Ives with a romantic conception of art music; and his wife, whose literary and philosophical interests had a powerful effect on Ives. Only about the time of his marriage in 1908, argues Burkholder, did Ives develop the mature philosophy of art that lay behind his most significant compositions, which began to appear about the same time. Rather than preceding the music, the philosophy may, in fact, have followed it.

Burkholder's evidence for the influence of various figures on Ives is not all equally convincing. But the chief disappointment for the professional historian in this book is its incomplete presentation of the social context of Ives's life. Ives's genteel family background, his status as a Yale man, and his career as a businessman placed pressures on him that were probably more important than his philosophy in shaping the course of his musical life. Neglecting this social context, Burkholder's discussion of the composer's aesthetic beliefs seems somewhat abstract.

FRANK ROSSITER
University of Texas, Dallas

PHILIP R. VANDERMEER. *The Hoosier Politician: Officeholding and Political Culture in Indiana, 1896–1920*. Urbana: University of Illinois Press. 1985. Pp. xii, 256.

Philip R. Vandermeer's book is a commendable endeavor to place an old subject—political leadership in the Progressive era—in a new perspective by focusing on state officeholding and its underlying

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political culture. Choosing to study Indiana as more or less typical of a broad range of midwestern and mid-Atlantic states, Vandermeer has written a collective biography of the 1,127 members of the General Assembly who served in that body during roughly the decade and a half after the election of 1896, with only passing attention to other politicians at the national, state, and local levels.

Before proceeding to an analysis of a "social profile" of state legislators, the author provides a brief but cogent description of the highly competitive, intensely partisan political climate in Indiana. The biographical data he has gathered from a variety of sources clearly indicate the great diversity of backgrounds as well as a few similarities, such as geographic stability, strong community ties, and certain occupational patterns. Not too surprisingly, Republican and Democratic lawmakers turn out to be more alike than different from each other, reflecting what Vandermeer calls "the consensus of society as to what politicians should look like" (p. 95). The chapter entitled "Race, Ethnicity and Politics" offers a refined version of the religious typologies that have been put forward in recent years to try to explain the connection between religious and political affiliations. The author's main conclusion is that the real distinction between political parties arose from a "division over basic values" (p. 145), which can be traced back, in large part, to differing religious orientations.

Despite a tendency toward political professionalization in this period, these Indiana legislators were mostly amateurs or "citizen politicians," whose careers in the General Assembly were comparatively short and who rarely went on to higher public office. Moreover, this study gives little support to the critical election theory, instead stressing the continuity of Indiana politics from the Gilded Age through the Progressive era. Whatever political change took place in this period was gradual rather than cataclysmic and derived chiefly from economic and demographic shifts. Yet the author does discern the slow emergence of a new political system in Indiana by the 1920s, marked by such features as the rise of professional politicians and a decline in party loyalty and identification. Perhaps an extension of his research into the next decade might prove useful in documenting this change and exploring further the political culture of the state and nation.

CLIFTON J. PHILLIPS
DePauw University

HENRY C. FERRELL, JR. *Claude A. Swanson of Virginia: A Political Biography*. Lexington: University Press of Kentucky. 1985. Pp. x, 294. $28.00.

Claude Augustus Swanson (1862–1939) enjoyed a long, productive, and influential career in state and national politics. Born and raised in the tobacco country of south central Virginia, he worked as a store clerk and school teacher before graduating from Randolph-Macon College and obtaining a law degree from the University of Virginia. He was elected to the U.S. House of Representatives from the Danville district in 1892 and soon emerged as a leading congressional advocate of low tariffs, currency inflation, rural free delivery of mail, and other popular causes. Victorious in Virginia's first statewide Democratic primary, Swanson captured the governorship in 1905 and commenced a four-year term that marked the high tide of Progressive reform in the Old Dominion. He displayed consummate skill in legislative coalition building and achieved substantial improvements in public schools, welfare services, railroad regulation, and the construction of farm-to-market roads.

Swanson shifted his energies to the U.S. Senate in 1910 and retained a seat there for more than two decades. Accumulating influence and seniority, he worked to enact Woodrow Wilson's "New Freedom" proposals, played a major role in support of the ill-fated League of Nations, and joined other farm-bloc senators to challenge successive Republican administrations during the 1920s. Throughout his senatorial career he championed the ship-building interests of Hampton Roads, gaining expertise that proved invaluable during a highly productive tenure as secretary of the navy (1933–39) in Franklin D. Roosevelt's cabinet.

Although far from uncritical, Henry C. Ferrell, Jr.'s assessment of Swanson's political activities is overwhelmingly sympathetic in tone. He demonstrates that Swanson was not an unprincipled minion of the "boss" of Virginia's Democratic party, Thomas S. Martin, as alleged by contemporary critics and echoed by such historians as William Larsen and Allen W. Moger. Ferrell's account also calls into question the image of Martin's (and Swanson's) Democratic "machine" as fundamentally backward-looking and traditionalist in orientation, an image advanced most forcefully in Raymond H. Pulley's *Old Virginia Restored* (1968). By contrast, the Claude Swanson depicted in Ferrell's text was neither a corporation-serving reactionary nor an ancestor-worshipping Confederate cultist. He was instead an astute, pragmatic politician with marked proclivities toward governmental activism and liberal reform, an ardent Wilsonian who became an equally ardent New Dealer.

Several flaws in Ferrell's study must be noted. The prose style is frequently wooden and sometimes opaque; excessive detail, especially concerning the minutiae of Virginia politics, occasionally impedes the flow of the narrative; a distressing number of

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typographical errors have survived proofreading. More significantly, Swanson's evolving (and intriguing) racial views could have been analyzed in greater depth.

Such shortcomings notwithstanding, the overall quality of this biography is high. Ferrell's interpretations are strikingly well documented and, for the most part, convincingly argued. His account reflects as well an impressive command of the historiography, issues, and personalities of a tumultuous half-century of American life—from the hard-fought political battles of the 1890s to the intramural clashes of the U.S. Navy's admirals and administrators in the 1930s. Without doubt, a significant career has found its definitive chronicle and assessment in Ferrell's book.

JAMES TICE MOORE
Virginia Commonwealth University

JEROLD L. WALTMAN. *Political Origins of the U.S. Income Tax*. Jackson: University Press of Mississippi. 1985. Pp. x, 124. $12.50.

The title of this brief book stands for a survey of the legislative histories and principal provisions of the first five federal revenue acts (1913–21) after the Sixteenth Amendment. It offers two principal conclusions about "political origins." The first is that, contrary to the old Progressive view, the statutory substance of tax policy issued in this period more from legislative contingencies and "peculiarities of each Congressional session" than from long-run social forces and political cleavages (p. 102). The second conclusion, for political scientists primarily, is that tax policy emerges from no single paradigmatic political process but that various sorts of process are characteristic, respectively, of various substantive aspects of tax policy—that is, they are associated with certain recurrent problems or conundrums of tax legislation.

Neither the legislative histories nor the principal conclusions can be pursued elaborately in just 120 pages of text and notes. The price of such brevity about something so intricate as income taxation is that the legislative histories are not at all self-evidently sufficient to the conclusions and that the conclusions seem too purely synoptic for persuasiveness. Generically, the second conclusion is broadly accepted in political science now, but one would like to know more about which aspects, among those identified by Jerold L. Waltman, tend to dominate tax policy and why. In addition, one would welcome a fuller defense of the centrality and generality of these elements. The first conclusion seems more questionable, because evidence to the contrary—especially that of the critical role in tax policy played by certain continuing cross-party alignments in the "fourth party system"—figures prominently in what seems to be Waltman's major concern, his formal "conclusions" notwithstanding. This has to do with how the income tax, which allegedly originated as a redistributive essay of the Populist-Progressive Left, became within eight years and even in postwar "normalcy" an essential source of federal (and intergovernmental) revenue and of public-sector expansion once its productivity had become apparent and its politics had shifted from redistributive to more strictly fiscal. In an ironic turn of the book's title, Waltman's explanation appears to emphasize technical rather more than political factors.

This book will be most useful for readers who, perhaps stimulated by the current quest for "simplification" and "level playing fields" in federal tax law, would like to learn how the tax law got so complicated. Waltman explains this well, if summarily, and the inner logic of his account suggests that simplification is unlikely to be carried very far or continued for very long. Specialists in this subject, and especially in this period, will probably find the book less useful.

CHARLES E. GILBERT
Swarthmore College

RICHARD H. COLLIN. *Theodore Roosevelt, Culture, Diplomacy, and Expansion: A New View of American Imperialism*. Baton Rouge: Louisiana State University Press. 1985. Pp. x, 246.

Anyone who consults *American Heritage Dictionary* under the heading "caricature" will find a familiar likeness of Theodore Roosevelt: all teeth and bristling with bellicosity. Happily, Richard H. Collin has joined a growing number of scholars engaged in the task of separating myth from reality. Readers will thus be treated to a rare view of the cosmopolitan Roosevelt. The same president who brought government into the field of labor-management relations during the anthracite coal strike of 1902 is here shown bridging the gap between Charles L. Freer and the Smithsonian to form a National Art Gallery. Roosevelt is also pictured restoring the White House to its original elegance and drawing together diverse leaders from the world of art, literature, and science to form a European-style salon. His own credentials as a Renaissance man are touched upon, and all in the context of an American "coming of age" symbolized by Chicago's "White City."

To be sure, there are omissions. The late nineteenth century saw American business penetrate a remarkable number of foreign markets. Overseas marriage and travel rose at an exponential rate while Americans such as Lafcadio Hearn and Ernest Fenollosa played key roles in the artistic life of other nations. The author bypasses most of this. Indeed,

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even a character as prominent as Edith Wharton finds no place as Collin skims lightly over the cultural scene, sketching here and there as fancy would seem to suggest.

Collin is at his best dispelling a host of stubborn myths. We are reminded that Roosevelt did not lust after the Philippines, that he had little to do with the outbreak of war with Spain, and that he was never imperialistic in the sense so often implied. Such findings may not be entirely new, but they offer a useful corrective and are presented with engaging panache. Needless to say, any attempt to banish old stereotypes will risk the creation of fresh ones, especially where generalization is insufficiently backed by evidence. Collin probably claims too much for cultural sophistication as a peace-keeping agent, placing it on a par with all other factors combined (pp. 52–54, 141). He is likewise on thin ice when he denies that Roosevelt spoke softly and carried a big stick (pp. 2, 176). No firm basis exists for the charge that Roosevelt wanted to kill "dagos" in Chile or that his attitude toward Canada was one of "measured contempt" (pp. 121, 173). Nor has it been demonstrated that Spain dealt in bad faith at all times during the Cuban crisis (p. 110). Finally, it would appear injudicious to dismiss the anti-imperialists as "simply racists" (p. 140).

On balance, Collin sets himself an ambitious goal that is not always fully realized. Nonetheless, he has given us a volume with many pleasant moments as well as some intriguing twists and turns, one that is well worth reading.

FREDERICK W. MARKS III
Forest Hills, New York

ROBERT C. HILDERBRAND, editor. *The Papers of Woodrow Wilson*. Volume 50, *The Complete Press Conferences, 1913–1919*. Princeton: Princeton University Press. 1985. Pp. xx, 826. $52.50.

It has become almost a cliché to comment on the high scholarly and production standards that have been maintained under Arthur S. Link's editorship in publishing *The Papers of Woodrow Wilson*. Volume 50 in the series, the transcripts of Wilson's press conferences, is no exception. Robert C. Hilderbrand, who edited this volume, started out already familiar with the transcripts from researching his book *Power and People* (1977). To put out an authoritative version, however, he had to go directly to the shorthand notebooks of Wilson's stenographer, Charles Lee Swem. This meant first of all learning the Gregg method Swem used and then engaging in painstaking effort to extrapolate what was said when Swem's notes went blank and to decipher his meaning when the notes were ambiguous. Hilderbrand has succeeded admirably, as he has in providing thorough annotation of who and what are being discussed. The result is a scholarly tour de force.

Wilson was the first president to hold regularly scheduled press conferences, and now that an authoritative record of those sessions is available we are in a better position to assess how well he did at them. The evidence is mixed. Hilderbrand points out that, on the one hand, journalists tended to write enthusiastically about the conferences at the time they were taking place, which is not surprising since Wilson comes across as articulate, well informed, and wide-ranging in the questions he fielded. On the other hand, the disparaging comments that popped up in journalists' memoirs, published mainly in the 1920s, are not altogether surprising either, considering that an undertone of presidential distrust, and even disdain, for the press was never far beneath the surface at those sessions.

The distrust probably had to do partly with Wilson looking down on people he regarded as his social and intellectual inferiors. That spirit is evident, for example, in his response to a questioner at one of the later conferences. "My dear boy," he said, "do you suppose I am going to tell you an answer?" (p. 781). More important, and it crops up repeatedly in the conferences, the tensions had to do with Wilson's annoyance at reporters for going beyond the hard facts he was willing to provide and pumping him for additional information or speculating about what might be. He regarded this tendency as particularly dangerous where diplomatic affairs were concerned. "The foreign policy of the government is the one field in which, if you will permit me to say so, you ought not to speculate about in public," he told the assembled press in early 1914 (p. 335). Since they persisted in doing so, the president's patience wore increasingly thin until he finally terminated the regular conferences in mid-1915.

Still, in his meetings with the press Wilson laid the foundation for an institution that remained important until television changed things at mid-century. It is good to have this record of how that institution began.

GEORGE JUERGENS
Indiana University

RICHARD WIGHTMAN FOX. *Reinhold Niebuhr: A Biography*. New York: Pantheon. 1985. Pp. x, 340. $19.95.

Reinhold Niebuhr was a religious thinker. His younger brother, H. Richard Niebuhr, tried to remind him of that fact and warned that his radical religion and radical politics were an unstable compound that could not sustain itself. But the older brother could not divorce his religious belief from political judgment, and among his several maxims

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was his insistence that, although there could be no ultimate fulfillment in the political realm, there could be no salvation without political commitment. And certainly no religious thinker in twentieth-century America won more recognition of his influence than did Niebuhr. Among those who have acknowledged him are Walter Lippmann, Arthur Schlesinger, jr., Hubert Humphrey, George F. Kennan, Martin Luther King, Jr., and Jimmy Carter. Much of the history of modern liberalism is centered in this group. But by the 1970s and 1980s, after a period of neglect, Niebuhr was being called to service as the theological voice of neoconservatism.

This excellent biography by Richard Wightman Fox, the first comprehensive account of Niebuhr's life, focuses carefully on the intellectual career of Niebuhr and details fully the expansive network of political and religious organizations that Niebuhr founded or led. Twelve chapters recount Niebuhr's life in strict chronological development and include fascinating glimpses of Niebuhr's family and personal relations; most revealing are those with his jealous brother H. Richard (Reinhold was the favorite of the Niebuhr children), with his protective mother, and with the opposite sex, to whom Niebuhr was aloof and unengaged. But withal the most suggestive and challenging theme in Fox's study is the place he assigns to Niebuhr in the tradition of Protestant liberalism.

The kind of liberal theology that had prevailed in Protestant seminaries since the late nineteenth century was under heavy assault in the post–World War I decades. American fundamentalists and Continental thinkers such as Karl Barth were attacking the rational, humanistic, scientific, and worldly character of liberalism. Protestant "neo-Orthodoxy," a label to which Niebuhr's name is always attached, emphasized a rediscovery of human sin, the need for grace, and a God of transcendence, judgment, and redemption as the authentic Christian view of life. Why then should Niebuhr, this scourge of liberalism, both political and religious, be moved over to the liberal tradition of American Protestantism?

Fox identifies Niebuhr's liberalism early in his life, in his efforts to bring the Evangelical Synod of his father's pulpit into the English-speaking fold of the American environment. World War I found Niebuhr embracing the American flag and championing Woodrow Wilson all through the crusade. Niebuhr was committed to Prohibition and his early sermons show a preoccupation with personality, two other liberal trademarks. In the 1920s Niebuhr's service at the Bethel Church in Detroit brought him into severe judgment against political liberalism as he attacked its idealism, sentimentalism, and hopelessly ineffective reformism in the face of industrial capitalism's monstrous moral evil. Niebuhr would soon see in liberalism a dangerous and short-sighted pacifism that further discredited it in an age of totalitarian cynicism. He would find his way by the 1940s to a cautious support of democracy and by the end of the decade to a militant anticommunist liberalism. But, through all these shifts and adjustments in Niebuhr's thinking, Fox finds that the religious perspectives that governed them confirmed his essential religious liberalism. Niebuhr, that is, stressed human experience over divine revelation, social reform over individual salvation, ethics over theology. Niebuhr simply came to use antiliberal means to liberal ends, and, as he sought to purge democracy of its naive intellectual foundations, so also did he endeavor to salvage a socially useful religious faith by grounding it in realism. Fox's framework is useful as long as it defines liberalism narrowly. Certainly, as he knows, Niebuhr never succumbed to the confidence that many religious liberals placed in science or the scientific method, and those who accused him of excessive rationalism failed to appreciate the ends of self-interest that Niebuhr always attributed to the human intellect.

For individuals who want a solid introduction to Niebuhr and others who have been long-time students of this voice of Christian realism, Fox has provided a valuable and rewarding book.

J. DAVID HOEVELER, JR.
University of Wisconsin, Milwaukee

WILLIAM YOUNG and DAVID E. KAISER. *Postmortem: New Evidence in the Case of Sacco and Vanzetti.* Amherst: University of Massachusetts Press. 1985. Pp. xiv, 188. Cloth $20.00, paper $8.95.

The most damaging piece of evidence tying Bartolomeo Vanzetti to the holdup murders of a paymaster and his guard at South Braintree, Massachusetts, in 1920 was the apparent similarity of the pistol he was carrying at the time of his arrest to the missing revolver of the slain guard. Although the prosecution could not positively identify Vanzetti's gun as belonging to the guard, it asked the jury to infer that this was the case and that the gun had been picked up at the murder scene by one of the bandits. Seven years later that argument could still impress the advisory committee headed by President Abbott Lawrence Lowell of Harvard. Now the files of the Massachusetts State Police, opened in 1977 under the Freedom of Information Act, prove conclusively not only that Vanzetti's gun was not the guard's but also that the prosecution had discovered this months before the trial. A report from the Bridgewater chief of police, who developed the case

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for the district attorney's office, reveals that he had traced the guard's gun to the place of purchase where its caliber and serial number were recorded. Since neither the caliber nor the serial number matched Vanzetti's pistol, it would appear that William Young and David E. Kaiser, the authors of this book, are justified in concluding that "the prosecution sent Vanzetti to the electric chair on evidence they knew to be false" (p. 58).

If District Attorney Katzmann and his assistant, Harold Williams (later a United States Attorney and justice of the Massachusetts Supreme Court), were capable of such duplicity in the matter of Vanzetti's gun, the grave charge made by the defense in 1927 and repeated in 1969 by Herbert Ehrmann (*The Case That Will Not Die*)—that the only autopsy bullet that matched Nicola Sacco's Colt automatic was not authentic but had been substituted for the original—becomes at least thinkable. Using grand jury testimony and notes made by Williams (on deposit in the Harvard Law Library), the authors proceed to an elaborate and ingenious reconstruction of the various paths the six murder bullets took to their targets. In the process Young and Kaiser convince themselves that only a single gun could have fired all these shots and that, therefore, the Sacco bullet must have been a plant. Any satisfactory substitution theory must meet Francis Russell's objections (*Tragedy in Dedham* [1962]), and it cannot be said this has been done here. But, if large implausibilities remain, they are not larger than those of the prosecution's case. At any rate, this book provides objective proof that the Commonwealth had so much to answer for in the conduct of its sworn agents that, whatever else is problematical, there is no doubt that justice was denied in Massachusetts.

FRED SOMKIN
Cornell University

SANDER A. DIAMOND. *Herr Hitler: Amerikas Diplomaten, Washington und der Untergang Weimars*. Translated by STEPHANIE KREUELS. Düsseldorf: Droste. 1985. Pp. 163. DM 44.

Sander A. Diamond stresses two points: first, that the Washington of the 1920s, preoccupied with German communism and German finances, overlooked the danger of the German nationalist and racist radical Right and, second, that this oversight occurred in spite of able and far-sighted analysis, at least in the earlier years, from American diplomats in Germany. Diamond's first point depends somewhat on his second. Monstrous though Hitler's doctrines were, it might seem to be wisdom by hindsight to expect the American government of the 1920s to worry about German radical racists. The British government apparently did not worry. But, if reports from Germany gave warnings, then indifference in Washington becomes less excusable.

Ambassador Alanson B. Houghton and others—notably, Vice Consul Robert Murphy in Munich—did take the Nazis seriously in 1922 and 1923, when Mussolini had recently "marched on" Rome, inflation was astronomic, and Bavaria in ferment. Some American diplomats believed then, as Diamond does now, that Hitler was receiving financial support from the heavy industrialists. Hugo Stinnes told Houghton in September 1923 that a dictator was needed and that "a man of this type was ready." Diamond italicizes these last words (p. 58), and he presents the story as confirmation that Stinnes supported Hitler. But the industrialist may have had the monarchist Gustav von Kahr in mind, and in any case Stinnes himself preferred a presidentially appointed dictatorial directorate, which he hoped would restore the ten-hour day.

While American leaders did abhor communism and did intervene financially in Europe, Diamond tends to overlook the real obstacles to *political* intervention. In the (retrospectively deplorable) view of the then-predominant "old stock" Americans, Europe was remote and its political—and ethnic and racial—problems were simply not America's concern. The presidents and senators (not the State Department) controlled policy, and they believed strongly in isolation. When informed of Houghton's reports of possible dictatorship in Germany, President Calvin Coolidge made a characteristically dismissive comment: "I do not feel that if certain people in Germany act foolishly, the result is going to be the downfall of civilization, as some people seem to think."

As to Diamond's claim that diplomats in Germany gave ample warning of radical nationalism and anti-Semitism, a check of some of his sources shows that he exaggerates this perceptiveness. In at least eight cases where Diamond credits American diplomats with reports on anti-Semitism, either the reports themselves do not contain such information or the reports simply cannot be found at or near the location Diamond gives.

Unfortunately, such nonsubstantiating or untraceable source references are a problem throughout this book. I ultimately checked some fifty-two source references and found only twenty that fully supported Diamond's statements. The author has been careless, and his defective references, however innocently made, lend only spurious support to his argument. Regrettably, this book cannot be relied on, and it does not advance our understanding of the 1920s.

EDWARD W. BENNETT
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EDWARD M. BENNETT. *Franklin D. Roosevelt and the Search for Security: American-Soviet Relations, 1933–1939*. Wilmington, Del.: Scholarly Resources. 1985. Pp. xix, 213. Cloth $30.00, paper $9.95.

Edward M. Bennett began his study of Franklin D. Roosevelt's policies toward the Soviet Union with a dissertation in 1961 supervised by Norman Graebner. In 1970, Bennett published a book on the period of U.S. nonrecognition of the Soviet Union and the recognition of Moscow in 1933. The book under review carries the story forward through 1940, and a forthcoming sequel will examine the Soviet-American alliance during World War II.

Bennett presents a familiar story, from Roosevelt's maneuvers that moved the United States toward recognition of the Soviet Union to the angry, emotional denunciations over the Soviet-Finnish war in December 1939. Since almost all of the important primary materials in this study were used in Bennett's dissertation, the book suffers from an absence of new information that might overturn previous assessments or offer fresh perspectives from the manuscripts of the participants. Bennett does carefully demonstrate that the expectations of recognition that Ambassador William Bullitt carried to Moscow were buffeted by the failure to resolve the debt issue, the violation of the agreement on noninterference by the Congress of the Comintern, and Roosevelt's and Stalin's inability to cooperate on the challenges posed by Japan and Nazi Germany. Bennett's coverage of the period 1936–39, which has received less scholarly attention, follows Roosevelt's unsuccessful, sporadic efforts to persuade the American public, as well as Secretary Cordell Hull and the State Department, to support bringing Stalin to the side of the democracies.

The most original contribution of Bennett's study is the careful focus on Roosevelt. The author has significantly advanced beyond the dissertation stage in this area. Recognizing the multifaceted characteristics of Roosevelt's approach to diplomacy, Bennett demonstrates that realism, pragmatism, idealism, and curiosity all played a role in the president's Russian policy. Bennett gives Roosevelt credit for having a better understanding than most Americans about the challenges the United States faced from Japan and Germany and for trying to bring Stalin's Russia into a cooperative relationship with the Western democracies against the Axis powers.

Although Bennett endorses Roosevelt's strategy, he criticizes its implementation. Responsibility for the ultimate failure of Washington and Moscow to cooperate is placed on all shoulders, from Secretary Hull and the State Department to Stalin and Roosevelt. Stalin's interest in cooperating with Roosevelt was undermined by Soviet misunderstandings and miscalculations, especially about Washington's need for a debt settlement before further advances toward cooperation could occur. Bennett criticizes Roosevelt for his failure to anticipate that it would take more than gestures and rhetoric to deter the Axis powers. Roosevelt's movement back and forth from proposals to drift is symptomatic of his diplomacy in the late 1930s, especially in his stance toward the Soviet Union. The final irony that Bennett stresses is that Roosevelt and Stalin wanted to block the aggressive expansion of Berlin and Toyko but neither leader could find a way to resolve their suspicions and disagreements to work out effective cooperation.

THOMAS R. MADDUX
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CHRISTOPHER BROOKEMAN. *American Culture and Society since the 1930s*. New York: Schocken. 1985. Pp. xv, 241. $22.00.

A reader who expects from this book a discussion of education, science, technology, journalism, film, sports, and political ideology—to list only a few of the subjects that might be called to mind by the phrase "culture and society"—will be disappointed. Christopher Brookeman deals with these matters only as they have themselves become the object of literary and cultural criticism. This is a book about cultural commentary, not one about culture and society.

Cultural commentary is an important subject, to be sure. It shapes cultural developments in its own right, and for this reason it would be inappropriate to insist on a strict separation of culture and cultural criticism, assigning the first to social or cultural historians and the second to intellectual historians. The tendency of recent historical scholarship is to break down such rigid classifications. A book about American culture and society since the 1930s would be deficient if it did not include some discussion of such figures as Dwight Macdonald, Robert Warshow, Norman Mailer, T. S. Eliot, and Susan Sontag. But it ought to include much else besides. Brookeman's study might better have been entitled *The Debate about Mass Culture*.

A few chapter headings will give an idea of the book's severely restricted scope: "Daniel Bell and the Cultural Contradictions of Capitalism"; "Robert Warshow and the Legacy of the 1930s"; "The Frankfurt School"; "The Politics and Aesthetics of Modernism"; "Coming to Terms with Hollywood: From Mass to Auteur Theory." It would be pleasant to report that Brookeman at least has fresh things to say about these familiar subjects, even if they are not the subjects his title leads us to expect. The conventional terms of debate about mass culture, which

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continue to govern historical scholarship on consumerism, work, and leisure, and even more distantly related issues such as "social control," need to be rethought, modified, or abandoned. A book that took up this task would have been welcome, but Brookeman merely summarizes various positions, deploys familiar quotations, and risks no judgments of his own.

The author not only avoids direct engagement with American culture but does not even engage his cultural commentators directly, relying instead on the work of other historians for most of his organizing ideas. He structures much of his narrative around the well-worn polarities of "commitment" and "conformity," vanguardism and cultural populism, liberalism and conservatism. The possibility that such interpretive categories will furnish new insights, at this late date, is remote.

The rarefied atmosphere that pervades Brookeman's survey is exemplified by statements like this: "Auteur criticism has not entirely closed the gap between high and mass art" (p. 178). One would not want to deny that criticism can have an important shaping influence on art, especially in an age when art and criticism often become indistinguishable. But criticism alone can hardly close the gap between high art and mass art—even allowing that this is a fruitful way to think about the problem in the first place. Such a gap is more likely to be closed by the industrialization of the publishing industry and by the relentless hype that now surrounds the marketing even of "high culture." A book such as Charles Newman's *Post-Modern Aura* (1985), which combines analysis of the economics of publishing with analysis of "postmodern" literary ideologies, throws more light on these matters than does the tiresome debate about "masscult" and "midcult," as does Thomas Whiteside's examination of the publishing industry, *The Blockbuster Complex* (1981). It is a pity that Brookeman does not venture into this territory, preferring instead to stick to the beaten path.

CHRISTOPHER LASCH
University of Rochester

RICHARD W. STEELE. *Propaganda in an Open Society: The Roosevelt Administration and the Media, 1933–1941.* (Contributions in American History, number 111.) Westport, Conn.: Greenwood. 1985. Pp. x, 231. $35.00.

It is no secret that Franklin D. Roosevelt developed informal, frequently held press conferences into something of an art form, touched millions of Americans directly with "fireside chats," and used both to polish his personal image and gain popular support for his policies. Nor is it often denied that he was the most cleverly manipulative of presidents. But no one has carefully examined the relationship between the Roosevelt administration and the media with a view to determining the extent and effectiveness of the manipulation that occurred. Richard W. Steele remedies this lack for the period to 1941.

The first of Steele's six chapters follows the pattern of Robert C. Hilderbrand's *Power and the People: Executive Management of Public Opinion in Foreign Affairs, 1897–1921* (1981) and sketches the mechanisms developed early in the Roosevelt administration for the executive management of public opinion. The second traces the evolution of relations between the White House and the press, from initial openness, cordiality, and cooperation to a far more guarded posture. Steele then describes the president's media efforts to meet "the challenge of isolationism" and devotes the final three chapters to separate examinations of newspapers, radio, and the movies in the eighteen months preceding U.S. entry into World War II.

Among Steele's more interesting findings are the extent to which "public relations" became an important element of government in these years and the real differences between the responses of the press, radio, and the movies to the administration's attempts to disseminate "facts" and win support. But, if the study is revealing in this and other respects, it is also inconclusive in important ways. Steele is careful in his assessments and makes no claim of a giant propaganda apparatus for the United States. But he has little to say on the effect of "propaganda" on domestic developments and is unable to specify to what extent the activities of the media in relation to foreign affairs resulted from conditions in the industries; the personal predilections of editors, broadcasters, and movie moguls; and pressure from Washington. He makes much of the fact that the vulnerability of broadcasters to FCC regulation and of film makers to antitrust action made both more willing to follow the administration's lead than the more secure press, and he gives some examples of pressures that were actually applied. But he identifies other factors that were in play and shows that the administration consistently shied away from strong-arm tactics and formal control.

If, as Steele concludes, the government's role in the process was one of "here providing information, there withholding it, everywhere encouraging, coordinating, facilitating, and discretely shaping what was by design a collaborative effort" (p. 172), then we are no closer than before to knowing whether the administration's actions were determinant in the events described, how effectively Roosevelt managed the public, or, for that matter, whether he remained within the bounds of persuasion and leadership permissible in a democracy.

MANFRED JONAS
Union College

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GARY E. KRASKE. *Missionaries of the Book: The American Library Profession and the Origins of United States Cultural Diplomacy*. (Contributions in Librarianship and Information Science, number 54.) Westport, Conn.: Greenwood. 1985. Pp. x, 293. $35.00.

The founding in 1942 of the Benjamin Franklin Library in Mexico City symbolized a remarkable period during which the American Library Association (ALA) pioneered in inaugurating cultural programs as an accepted part of U.S. diplomatic relations. To an extent that may surprise generations accustomed to direct government action, the ALA, a wholly private organization operating with Rockefeller Foundation and U.S. government funding, acted during the years between 1938 and 1949 as the chosen instrument of government in projects that were seen as reflections of official policy overseas. This period, a time of trial for projects and for policies, is the focus of Gary E. Kraske's excellent study. The title of his dissertation, "The American Library Association in the Emergence of U.S. Cultural Diplomacy, 1938–1949" (Columbia University, 1983), well expresses the general scope of the work.

After an introduction, Kraske considers the long history of U.S. relations with Latin America as a prelude to dealing with different aspects of ALA's involvements. He focuses specifically on the projects in Latin America; the extensive, though mostly abortive, efforts to help China in wartime; and the multitude of activities related to European libraries and the outposts of the British Commonwealth. He then describes the export of books around the world and the ALA's assistance to library education in other lands. He ends with a discussion of the association's international efforts after the war and a general assessment of the whole experience. Throughout the work, he introduces new topics with background summaries that are often masterly in the clarity of their explanations.

For book publication, the text has been rather extensively copyedited and somewhat revised and reorganized. The changes are improvements, and the deletions are primarily details for which only the most diligent scholar will need to consult the original. Kraske bases his study on sound use of primary and secondary sources—notably, the ALA archives preserved at the University of Illinois at Urbana, the records of the Rockefeller Foundation, and U.S. government files.

Substantively, the study deals with significant issues. The ALA's work set important precedents, although later government actions have not always followed them. A major choice has always been between the librarians' conviction that the best policy is to provide demonstrations of excellent library service and the government officials' view that book and library programs should directly serve ideological goals by showing the United States only in the best light.

This excellent work makes a sound contribution to the history of U.S. involvement with international cultural relations.

W. L. WILLIAMSON
University of Wisconsin

HASKEL LOOKSTEIN. *Were We Our Brothers' Keepers? The Public Response of American Jews to the Holocaust, 1938–1944*. New York: Hartmoer. 1985. Pp. 287. $18.95.

This book is part of a growing literature charged by an unhappiness with the role as witnesses that American Jews played during the Holocaust. Haskel Lookstein finds that American Jewry did not really care about the fate of its European brethren, that American Jews were less than their brothers' keepers. This serious charge cannot be completely substantiated, as Lookstein attempts to do, by culling quotations from a sampling of the Yiddish and Anglo-Jewish press. What he presents does not have the integrity of a full survey of the Jewish press in America, nor does it yield a balanced account of the American Jewish reaction to the destruction of European Jews. When the distressingly thin veneer of "scholarly research" is pulled away, one finds a polemic composed by someone who is unhappy with what American Jewry was during those bitter years.

The selected newspapers and journals of the period reveal a Jewish American subculture so variegated that the basic assumption of this work is doubtful: did a collectivity that was capable of unified action actually exist? Lookstein never encountered this dilemma because his survey is not continuous or systematic. Instead, the laborious process of monitoring even the few journals selected is short-circuited because the author examines reactions to only six incidents: *Krystallnacht*, the St. Louis episode, confirmed news of the Final Solution, the campaign to create a government rescue agency, and the Hungarian episode. The narrative is fleshed out by quotations from a narrow group of secondary works. Quotations from my own work are usually cited out of context. Studies requisite for a balanced judgment are conspicuous by their absence.

A conclusion that American Jews failed to act responsibly toward European Jews cannot be drawn without confronting the question of whether American Jews had sufficient power to do so. Lookstein is aware of the virulent anti-Semitism of the period but remains oblivious to the equally important external and internal inhibitors that compromised the ability of Jewish leaders to alter the "do-nothing" policy of the Roosevelt administration. "The final solution," he admits, "may have been unstoppable by American Jewry, but it should have been *unbearable* for them. And it wasn't" (p. 216). Were

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Lookstein a more energetic and skillful researcher, the charge of "not caring" would still remain unsubstantiated. It lends itself far better to description of postures adopted by individuals than by entire communities. Moreover, for a historian, what a group believed is far more difficult to discover than what it did. That requires assiduous and careful research to find the tracks of a mental attitude and a balanced mind-set to evaluate such data. There is no evidence of these requisites in this work.

HENRY L. FEINGOLD
Baruch College
City University of New York

DONALD E. COLLINS. *Native American Aliens: Disloyalty and the Renunciation of Citizenship by Japanese Americans during World War II.* (Contributions in Legal Studies, number 32.) Westport, Conn.: Greenwood. 1985. Pp. 218. $29.95.

William Hohri first brought Donald E. Collins's book to my attention in the newsletter of the National Council for Japanese American Redress. Hohri was put off by the book's slender size, substantial cost, and reliance on secondary works. But what seriously disturbed him was the author's thesis that most of the 5,589 American citizens of Japanese ancestry who, during a three-month period in 1944–45, renounced American citizenship did so because they were "reacting to real and imagined events which were often beyond their control" and not because they were "truly disloyal to their country" (p. 4). Although well intentioned and persuasive, argued Hohri, this thesis was profoundly wrongheaded in that it legitimated an issue—loyalty—that the government had trumped up after Pearl Harbor as a rationale for its manifestly unconstitutional incarceration of 112,000 Japanese-Americans. However unwittingly, Collins's line of reasoning simultaneously served to mask the government's illegal, racist actions and mischaracterize the renunciants as victims without victimizers. Although the book was an informative and sympathetic study, then, it was not one that Hohri in good conscience could recommend.

Although largely conceding the force of Hohri's central objection to the book, I believe Collins makes a significant contribution both to American legal history and Japanese-American studies. Much of the ground in his well-crafted and readable book has been covered before—most notably, in Dorothy S. Thomas and Richard Nishimoto's *The Spoilage* (1946) and Michi Weglyn's *Years of Infamy* (1976). Still, this is the first systematic synthesis of the events that prompted one out of every fourteen Japanese-Americans to renounce their citizenship during World War II and the postwar legal actions to restore the citizenship of all but a small fraction of the renunciants.

I do have some reservations about the book. First, the sociocultural context that Collins provides for understanding why "a sizable minority [of Japanese-Americans] broke under the strain and renounced their American citizenship" is too constricted (p. 5). Collins is practically silent on potent forces that prompted the passage of the Nationality Act of 1940 and the amendment to that act (1944), whereby American nationals could renounce their citizenship during wartime while still residing within the United States. Second, the middle five chapters of the book—covering the 1943 registration crisis at the Tule Lake camp and the subsequent rise there of coercive chauvinistic factions once it became a segregation center for alleged pro-Japanists and their families—represent little more than a rehash of easily accessible secondary accounts. Collins should have either curtailed this section or, preferably, plunged into the primary material and staked out his own interpretive perspective.

The last two chapters, centering on the belated reaction by the renunciants under "normalized" circumstances to their earlier decisions and the ensuing battle to restore their citizenship, strike me as schematic and distanced. Although Collins informs us that "the renunciants' letters told pitiful stories relating the reasons for their renunciations and begged for relief from the hardships caused by their hasty actions" (p. 110), he does not integrate these stories into his narrative; rather, he decontextualizes and depersonalizes them in an appendix. The author interviewed Wayne Collins (no relation), the fiery, indefatigable San Francisco attorney who almost singlehandedly championed the cause of the renunciants, but did not interview any of the renunciants themselves or even consult the voluminous published testimony from the 1981 congressional hearings of the Commission on Wartime Relocation and Internment of Civilians. Had he done so, I doubt if, without accompanying comment, he would have cited Wayne Collins's 1972 statement: "I am the only person in the world who is still angry [about wartime treatment of Japanese-Americans]" (p. 4).

If the historiography of this book is slightly stale and its analytical framework too narrowly legalistic, perhaps it is because Collins reworked his dissertation for publication at this propitious time when Japanese-Americans are seeking redress for wartime injustice. Nevertheless, this book will provide students of the renunciation issue with a solid foundation on which to construct more conceptually innovative and socially grounded studies of this crucial and consequential topic.

ARTHUR A. HANSEN
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E. B. POTTER. *Bull Halsey*. Annapolis, Md.: Naval Institute Press. 1985. Pp. xiii, 421. $19.95.

This is not a scholarly biography but an affectionate portrait of a World War II hero drawn by an artist mellow in years and rich in skill. It limns the career of Fleet Admiral William Frederick Halsey (1882–1959), whose naval service ran from McKinley's second inaugural parade through Japan's surrender aboard the USS *Missouri*.

E. B. Potter creates three overlapping images of Halsey. The first is "Bull," the tough leader whose language, defiance of the Japanese, and consideration for his men made him an inspiration to the generation that fought the Pacific war. The second is "Bill," the midshipman football hero, ardent suitor, hard-drinking junior officer, and skilled manipulator of men and rules to his advantage. With zest and humor Potter portrays the fifty-one-year-old Halsey taking flight training, spurning eyeglasses to camouflage his poor eyesight, and piloting aircraft in a zany and dangerous manner—all so that he might become an effective aircraft carrier commander. The third image is that of Halsey the fleet commander of World War II, a man forced by new technologies, enormous distances, and poor communications to make critical—and controversial—decisions.

Potter is at his best in portraying Halsey the commander. Combining oral histories, his own earlier reconstructions of major naval battles, and his familiarity with those who served under Halsey, his narrative has an immediacy that greatly augments understanding of the admiral's major decisions at Guadalcanal, in the Solomons campaign, and at Leyte Gulf. Although critical of Halsey in the last engagement, he blames poor communications with army and navy commanders—for which the admiral was not responsible—for his sporadic pursuit of Japanese forces.

Potter does not accept uncritically the media-enhanced portrait of "Bull" the hero. He reveals how cautious the admiral was in hazarding carriers and how wrong and inappropriate his promises of early victory were. Potter's treatment of "Bill," though capturing the spirit of the man, does not probe deeply into Halsey's darker side. One would like to know what it was about this man, who drank ten cups of coffee and smoked forty cigarettes a day, that led to his wife's depression and her eventual separation from him and to his son's defiant criticism of his performance at Leyte Gulf.

General readers and specialists alike will appreciate this superb portrait—the former for its color, the latter for the broader analytical reflections about the nature of the Pacific war, which it invites. When hung alongside similar word-pictures of American, Japanese, and British admirals, it points to the need for a collective portrait of the naval commanders of that great conflict.

ROGER DINGMAN
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RONALD H. SPECTOR. *Eagle against the Sun: The American War with Japan*. (Vintage Books.) New York: Random House. 1985. Pp. xvi, 589. $9.95.

With this new synthesis of the American war against Japan, Ronald H. Spector contributes to the revival of U.S. military history as a respected research and teaching field in the post-Vietnam era. He combines traditional battle history with the latest research on radio intelligence and incorporates the newer social history that focuses on black GIs, women in the armed services, and the cultural interaction of U.S. soldiers and Pacific natives.

Evaluating the vast literature on the Pacific war of 1941–45, Spector arrives at balanced conclusions on whether Franklin D. Roosevelt had prior knowledge of an attack on Pearl Harbor and on the motivations for the decision to drop two atomic bombs on Japan. Likewise, Spector provides an even-handed treatment of the still-sensitive issues surrounding Admiral William Halsey's and Raymond Spruance's questionable and costly wartime command decisions.

Spector makes a major contribution to U.S. military history and to the Wars of the United States series by conceptualizing the Pacific war in an interservice setting. Never before had war demanded such large-scale coordination and cooperation among land, sea, and air forces. Yet separate army and navy traditions, organizations, and doctrine interfered with everything from prewar planning to overall strategy, from aerial operations to amphibious landings, and from the employment of intelligence to the distribution and allocation of supplies. The war became a struggle to overcome not only the enemy but also interservice disputes, rivalries, and bottlenecks that plagued both sides. Ultimately, the United States was better able than was Japan to find creative ways to bypass, though never to solve, interservice differences and to win the war.

The interservice framework becomes so pervasive, indeed, that at times it entangles Spector, who determines that only the U.S. Marine Corps, of all the military services, held a realistic doctrine and a viable approach to Pacific combat operations. Despite this minor criticism, *Eagle against the Sun* establishes Spector as one of the best of a new generation of American military historians.

JEFFREY M. DORWART
Rutgers University, Camden

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RONALD SCHAFFER. *Wings of Judgment: American Bombing in World War II*. New York: Oxford University Press. 1985. Pp. xiv, 272. $18.95.

In this book Ronald Schaffer inquires into the extent to which American political and military leaders were concerned with the moral issues involved in strategic bombardment during World War II. The crux of the matter was the inaccuracy of large, long-range aircraft carrying heavy loads of bombs: bad weather, human error, enemy interference, and other reasons made it impossible to guarantee hitting only precise targets. As a consequence, missions designed to strike specific military objectives, such as oil refineries, transportation facilities, and factories producing war goods, often resembled indiscriminate attacks or terror bombing on enemy populations. Late in the campaigns against Germany and Japan, despite the desire to avoid inflicting casualties among women and children, the driving necessity to win the war as quickly as possible led to air operations against urban centers where industrial plants and other legitmate targets were located. Military commanders justified the resulting casualties among civilians on the basis of breaking the enemy's will to resist, thereby shortening the war and saving American lives. Thus, the bombing of Dresden and the fire bombings of Tokyo occurred. The culmination came with the atomic bombs dropped on Hiroshima and Nagasaki.

After carefully investigating official documents, mainly those of the U.S. Air Force, Schaffer finds it "extraordinary" that "in the midst of a savage, atrocity-filled war, American generals based military decisions at least partly on moral concerns" (p. ix). This somewhat naive conclusion should come as no surprise. The generals, as well as the scientists and the political leaders, wished not only to overwhelm their adversaries but also to preserve the United States as the protector of civilization and humanitarianism. If the air force chiefs wanted to prove the efficacy of air power, they sought also to avoid tarnishing the postwar image of their service. Strategic bombing, they tried to show, was a destructive force but, in their hands, not wantonly so.

The epilogue traces briefly the evolution of American air-war planning in the postwar years. Earlier wars in history, Schaffer says, were like games, fought with rules and boundaries as well as with respect for noncombatants. These restrictions, he adds, began to dissolve when Italian bombers struck Ethiopian villages, when German planes destroyed part of Rotterdam, when the Germans fired rockets to kill and injure indiscriminately. In World War II, "moral compunctions against city bombing . . . disintegrated," and a "moral revolution in warfare" took place (pp. 217, 218). The perils of unchecked technology, the author warns, threaten us all today.

A sober, thoughtful account, always readable and occasionally repetitive, this book discounts the unified will of the American people during the Second World War, a total war for total victory. What bothers the author, with some justification, is evidence that the attitude of that time, which can now no longer be maintained because of advanced technology and potential conflict, still seems to prevail.

MARTIN BLUMENSON
Washington, D.C.

PAUL BOYER. *By the Bomb's Early Light: American Thought and Culture at the Dawn of the Atomic Age*. New York: Pantheon. Pp. xx, 440. $22.50.

At the beginning of his book Paul Boyer indicates his debt to Merle Curti. It is most appropriate that he should do so, because *By the Bomb's Early Light* is squarely within the Curtian tradition in American intellectual history. Boyer presupposes that what he variously terms "the nation's mood" (p. 48), "the American mind" (p. 340), and the "collective consciousness" (p. 279) exists as an apprehensible, if but slightly quantifiable, entity. By surveying a wide-ranging body of contemporary materials—books, articles, editorials, letters to editors, radio broadcasts, motion pictures, popular music, opinion polls, the personal papers of a number of prominent public figures—Boyer seeks to understand how the atomic bombs dropped on Japan in 1945 and the possibility of the devastation of the United States by the new weapon affected the thinking of Americans in the first five years of the atomic age. As Boyer demonstrates, all the major aspects of today's debates about nuclear armament and disarmament were manifest soon after the destruction of Hiroshima. Focusing on the period up to the Truman administration's initation of hydrogen "superbomb" development in 1950, Boyer seeks to "recover this lost segment of our cultural history," without which, he believes, "we cannot fully understand the world we live in, nor be as well equipped as we might to change it" (p. xix).

Boyer examines articulated opinion in the post-Hiroshima years from almost every possible angle: initial reactions; calls for a world government to avert future atomic wars; the rise and decline of political involvement among atomic scientists; popular visions of an atomic utopia; implications of the bomb for society, morals, and attitudes toward science; and efforts by government and allied groups to promote public acceptance of the bomb's military necessity and of atomic energy as a positive, benevolent factor in people's lives. Those efforts, Boyer believes, were generally successful. "The dread de-

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stroyer of 1945 had become the shield of the Republic by 1950" (p. 349). An epilogue sketches the ebb and flow of antinuclear activism since the 1950s.

Referring to post-Hiroshima claims by various psychologists that feelings of guilt and fear were widespread among Americans, Boyer admits that "such . . . generalizations about an entire society are, by their very nature, unprovable" (p. 182). That caution might well be applied to Boyer's entire book. What he has given us is a highly perceptive, well-researched, and eloquent, often passionate, account of how a minority of Americans—those possessing sufficient public concern to have something to say and sufficient verbal skills to be able to say it—dealt with the profound and haunting reality of the atomic bomb. But, if neither Boyer nor anybody else can tell us what Americans as a whole have been thinking or feeling, he has nevertheless written a book that should be read by all those who want to know how the argumentative framework of the 1980s on nuclear policy reached essential form four decades ago.

CHARLES C. ALEXANDER
Ohio University

GEORGE T. MAZUZAN and J. SAMUEL WALKER. *Controlling the Atom: The Beginnings of Nuclear Regulation, 1946–1962*. Berkeley and Los Angeles: University of California Press. 1984. Pp. x, 530. $28.95.

This book provides a detailed survey of the development of regulatory policy by the Atomic Energy Commission (AEC) before 1962. The authors state that their purpose is to write public history in order to provide the Nuclear Regulatory Commission (NRC) and the general public with an historical overview of the early regulation of nonmilitary nuclear power. The authors exclude discussion of the AEC's promotional activities, concentrating instead solely on regulation. Moreover, except for the fallout controversy generated by the atmospheric testing of nuclear weapons, they also ignore the military applications of nuclear power, including questions of foreign and military policy, no matter how germane these might be to the regulation of civilian applications.

Controlling the Atom is a "house history"; George Mazuzan and Samuel Walker are both professional historians employed full-time by the NRC. They enjoy access to some of the principals in the story they relate and to auxiliary research services provided by NRC staffers, including assistance in declassification. But the Washington axiom, "where you sit is where you stand," also holds true of historical writing. As insiders, the authors reflect the attitudes of their agency. There is no impropriety in this, of course. The authors insist that the book is the product of "our independent professional judgment" (p. viii), but their independent judgment turns a blind eye to the AEC's mendacity, blunders, and cover-ups. For example, they note widespread challenges, including one by the United States Weather Bureau, to the AEC's and Commissioner Willard Libby's claim in 1957 that radioactive fallout from weapons testing would be uniformly deposited over all the earth's surface (p. 51). But they fail to follow that up with the information that the AEC and Libby were wrong and their critics correct. An uninformed reader is left to surmise that the dispute ended inconclusively.

The authors' narrow and arbitrary compartmentalization of their topic leads them to omit material that most observers of nuclear affairs would think essential. We find no mention, or only fleeting notice, of the Manhattan Project and General Leslie Groves, J. Robert Oppenheimer (and his security hearings in 1954), McCarthyism, Enrico Fermi, Julius and Ethel Rosenberg, Linus Pauling, Ernest O. Lawrence, the House Committee on Un-American Activities, the economics of nuclear-generated electrical power, the controversy in Congress about public versus private power in the mid-1950s, the Windscale nuclear accident in 1957, and the international nuclear marketing offensive mounted by public and corporate officials.

This is a history of a regulatory bureaucracy and not of the men who ran it. Consequently, the principal figures appear as bloodless shadows. The arrogant, petty, vindictive AEC Commissioner Lewis Strauss seems a cipher: his venomous feud with Senator Clinton Anderson is left largely unexplained, though noted in passing. Because the authors focus on the AEC, all other significant institutions fade into the background. The Joint Committee on Atomic Energy and, behind it, Congress itself become a mere foil or nuisance to the AEC; the executive branch, the national laboratories, the universities, and the nuclear-industrial complex are mute spear carriers forming a backdrop on the stage.

Within its constrictive focus, this is a thorough bureaucratic history. Readers seeking a comprehensive review of the regulation of nuclear power's civilian applications, written by authors who maintain a critical distance from their topic, will be disappointed.

WILLIAM M. WIECEK
Syracuse University

GREGG HERKEN. *Counsels of War*. (Borzoi.) New York: Alfred A. Knopf. 1985. Pp. xvi, 409. $18.95.

In contrast to his *Winning Weapon: The Atomic Bomb in the Cold War, 1945–1950* (1980), which focused on

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government policy makers in the Pentagon and State Department during a brief five-year period, Gregg Herken in this new study adopts a far broader approach, examining the ideas of a large company of civilian defense intellectuals who helped shape American nuclear strategy during the forty-year interval when the nation's arsenal grew from three primitive atomic bombs to many thousands of warheads and missiles.

The general contours of Herken's account are familiar. He traces the history of deterrence doctrine as first articulated by Bernard Brodie in 1946, formally proclaimed by Secretary of Defense Robert McNamara in the 1960s, and ratified by the superpowers in the Antiballistic Missile (ABM) Treaty of 1972. He then explores the challenges to deterrence theory and its underlying counter-cities premise by strategists who argued that the United States must develop and adopt as its declaratory policy a nuclear warfighting capacity directed against the Soviet military—a doctrine espoused by Secretary of Defense James Schlesinger under the rubric "flexible response" in 1974.

If the broad outlines of the story are familiar, Herken adds great depth of detail, exploring the complex strategic controversies of the past four decades almost blow by blow. Examining the world of think tanks such as California's RAND Corporation and of Harvard and MIT strategists in their endlessly proliferating task forces, consulting teams, and study groups, Herken conveys more fully than has anyone else the intense social environment within which the strategic process has gone forward. For all their doctrinal differences, the members of this fraternity (and this *is* an almost exclusively male world) constitute a tightknit and influential group as they move from government to campus to research center to government. The richness of detail with which this process is conveyed rests on prodigious research, including more than sixty interviews with, among others, "three former secretaries of defense, four ambassadors, seventeen nuclear strategists, and twenty-two atomic scientists" (p. xi).

An illuminating and important work, this is also in some respects a frustrating one. Herken at times becomes immersed in minutiae, and the larger focus is blurred. The chapter titles, chosen for literary effect rather than descriptive clarity, do not much aid the reader seeking guideposts along the way. Bothersome errors of detail intrude: President Truman's press secretary Charlie Ross is confused with adviser Charles S. Murphy, Brien McMahon's name is slightly misspelled, and the author of *Alas Babylon* is given as Frank Herbert rather than Pat Frank. More troubling still is the erratic documentation. Key passages and even direct quotations are often footnoted ambiguously or not at all. The documentation for some of the most interesting material and quotations consists of the single frustrating word "Interview"—a concession Herken had to make to secure the cooperation of some highly placed interviewees.

For the most part, except for Vietnam, the strategic process is examined without much reference to the larger political, economic, and public context. To a degree, this accurately reflects the extent to which this process has, in fact, been isolated from the larger society. But in part it reveals a gap in the interpretive framework. In particular, one would have welcomed more systematic attention to the influence of scientific research and technological breakthroughs in propelling the strategic process forward and to the role of the major defense contractors in funding strategic studies.

On balance, however, this is a highly valuable work that merits careful attention. It helps illuminate and demystify the Alice-in-Wonderland world of nuclear strategy in which each new doctrinal permutation spawns its antithesis, which then becomes the basis for a new round of strategic cogitation, while the world's nuclear arsenals grow and prospects for meaningful arms control recede further and further into the distance.

PAUL BOYER
University of Wisconsin

GEORGE H. CALLCOTT. *Maryland and America: 1940 to 1980*. Baltimore: Johns Hopkins University Press. Pp. xii, 376. $27.50.

George H. Callcott has superbly met the challenge that the title of this work implies. "The theme of this book . . .," as he notes in the preface, "is American history working itself out in the moods and problems of Maryland. More specifically, the theme is the suburbanization, bureaucratization, and modernization of America as it occurs in Maryland, transforming politics, culture, and way of life" (p. ix). Under these broad topics the author, in some fifteen chapters, deals with such subjects as the four cultures of the state (Baltimore City, Eastern Shore and southern Maryland, western Maryland, and suburban counties of Baltimore and Washington, D.C.); Maryland in World War II; the administrations of President Harry S Truman and Governor William P. Lane; communism in Maryland; the black revolution; poverty and the welfare state; Spiro T. Agnew's crusade for good government; education (progressivism, integration, radicalism, and accountability); the administrations of President Richard M. Nixon and Governor Marvin Mandel; and the respites from more governmental programs, which characterized the administrations of Presidents Gerald R. Ford and Jimmy Carter and Acting

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Governor Blair Lee III and Governor Harry Hughes.

This book is a delight to read, for Callcott not only writes with grace but also sets before the reader a wealth of significant information. His observations on movements, trends, and institutions are both balanced and insightful. His capsule portraits of such gubernatorial figures as Theodore R. McKeldin, J. Millard Tawes, Mandel, and Lee are drawn with an understanding, though perceptive, touch.

With an attractive format, the book has an abundance of interesting tables, figures, and photographs and includes a bibliographical note, endnotes that reveal the author's comprehensive command of the sources, and an index. It is virtually free of factual and typographical errors. (An exception appears on page 257: by 1980 there were seventeen community colleges in Maryland, not twenty-two as reported.)

In his closing lines Callcott, with just the right touch, adjures: "Look up, and Maryland history is about all of America. Look down, and Maryland history is about the moment and one's self" (p. 317).

WILLIAM LLOYD FOX
Montgomery College

LEE SEIFERT GREENE. *Lead Me On: Frank Goad Clement and Tennessee Politics*. Knoxville: University of Tennessee Press. 1982. Pp. xviii, 555. $19.95.

TOM WAGY. *Governor LeRoy Collins of Florida: Spokesman of the New South*. University: University of Alabama Press. 1985. Pp. x, 264. $22.50.

Frank Goad Clement of Tennessee and LeRoy Collins of Florida were two of the most promising southern governors in the 1950s. They had a good deal in common, including military service in World War II. They were both young, dynamic, and politically ambitious. Although both lived in the peripheral South, they were products of the region's traditional one-party politics, the Tennessean being identified with the conservative wing of the Democratic party in the Volunteer State and the Floridian representing the more progressive elements in his state's amorphous Democratic factionalism. Both men were racial moderates and self-styled reformers. Both sought national recognition. Clement was selected as the keynote speaker at the Democratic National Convention in 1956, and Collins served as permanent chairman of the party's national convention four years later. Both southerners were prominently mentioned as possible vice-presidential candidates. But in the 1960s their influence waned, and crushing defeats at the polls ended their political careers. Clement was a victim of his state's virulent intraparty factionalism and of a resurgent Republicanism, as well as his own mistakes and voter weariness with a politician who went "to the well too often." Collins's downfall was largely the result of his identification with racial reform. He had, as Tom Wagy remarks, outdistanced the constitutency that sustained his career.

Lee Seifert Greene has written a comprehensive account of Clement's public life. He describes the politician's emergence as a serious political contender on the basis of attractive personal qualities, old-fashioned oratory, and such unabashed appeals as the one he made to all "who remember Mother's tears as the silver tide upon which the ship of state can most safely sail . . . to all who could say precious Lord take my hand, lead me on" (p. 65). In 1952 Clement won the Democratic gubernational nomination in a hard-hitting campaign against the Democratic incumbent, was overwhelmingly reelected in 1954, and, after being out of office for four years, was elected to a third term in 1962. He served a total of ten years. Despite his conservative affiliations, including the Crump organization in Memphis, the young governor fulfilled many of his promises. He sponsored a notable expansion of state services in areas such as education and mental health, as well as increased aid to municipalities. He followed a moderate course in dealing with school desegregation, acted forcefully to put down mob violence, took steps to modernize the state government, and tried unsuccessfully to secure repeal of the Tennessee right-to-work law. But Clement was no friend of legislative reapportionment; he made use of traditional patronage methods, relied on regressive taxes, and cooperated with special interests such as the powerful trucking industry. In 1958 he helped elect a conservative lieutenant named Buford Ellington as his successor, and their "leapfrog" administrations controlled the Tennessee governorship from 1952 to 1970. Greene's biography, which was commissioned by Clement's family and friends, reflects diligent research and extensive use of interviews, newspapers, and the Clement papers. Greene is more critical of the governor's opponents, particularly Senator Estes Kefauver, than of Clement. He avoids discussing Clement's drinking problem and certain other aspects of his personal life that were widely talked about by contemporaries. Nevertheless, Greene's treatment of Clement and the race question is illuminating, and he clarifies the uneasy alliance between Clement and Ellington.

The second of these books is based on Collins's gubernational and personal papers and on a variety of other sources. Less comprehensive than Greene's biography of Clement, Wagy's study concentrates on Collins's governorship during the years 1955–61, on his role in the Democratic National Convention of 1960, and on his work as director of the Community Relations Service following passage of the Civil Rights Act of 1964 and, subsequently, as Undersec-

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retary of Commerce. Wagy characterizes Collins as a "New South" politician in the tradition of the "business progressives" of the 1920s. As governor, Collins was an advocate of constitutional reform, legislative reapportionment, the expansion of education, economic development, and conservation. Like Clement, he opposed capital punishment. He was increasingly preoccupied with school desegregation and race relations, and it is in discussing this theme that the author's study is most impressive. Although Collins pledged himself to defend segregation and signed a pupil placement bill, he opposed extremism and violence, used his influence against school-closing legislation, and prevented passage of an interposition resolution. During the Little Rock school crisis, he took a leading part in efforts to work out a moderate solution and opposed a resort to massive resistance. As time passed, his views gradually changed, and he eventually acknowledged that the civil rights movement involved "moral rights" and "principles of brotherhood" as well as legal claims.

Although differing in some respects, Clement and Collins were both transitional figures in an era of upheaval and incipient change in southern politics. They were precursors of a new breed of southern governors typified in the 1970s by such politicians as Reubin Askew, Jimmy Carter, and Dale Bumpers. Largely emancipated from the burden of race, these more recent southern leaders were interested in creating broader (and often interracial) coalitions, in modernizing the instruments of government, and in promoting rapid economic growth. The appraisals offered by Greene and Wagy are suggestive in this respect, as in others. But neither work does justice to its subject. Clement's politics deserves more critical and incisive examination, and Collins's political campaigns, legislative initiatives, and executive leadership need more thorough analysis. Nevertheless, these volumes are useful and make a scholarly contribution of some importance.

DEWEY W. GRANTHAM
Vanderbilt University

STEVEN F. LAWSON. *In Pursuit of Power: Southern Blacks and Electoral Politics, 1965–1982*. (Contemporary American History Series.) New York: Columbia University Press. 1985. Pp. xix, 391. $30.00.

Near the end of his thoroughly researched, well-written book, Steven F. Lawson quotes the 1976 election-night fears of then Georgia congressman Andy Young, as Jimmy Carter faltered in the West: "I heard that it may depend on how Mississippi went and I thought 'Lord have mercy.' But when I heard that Mississippi had gone our way, I knew that the hands that picked the cotton finally picked the president" (p. 256). The mathematics of Carter's victory is susceptible to more complex interpretation, but Carter's 90 percent support among Afro-American voters in the South was unquestionably among the deciding factors. Thus, the Young quotation strikingly confirms Lawson's major premise that the Voting Rights Act of 1965 and its two extensions (1975, 1982) have politically empowered Afro-Americans to an impressive degree and irreversibly altered southern politics.

The battle to entrench minority voting rights has overcome politicians as diverse as Henry Hyde, Strom Thurmond, and Ronald Reagan, who now seem reconciled to living with at least the forms of the new arrangements if not the spirit. "There have been violations of voting in the Southern states in years gone by. To some extent we have to pay for that," Louisiana senator Russell Long conceded at the time of the last extension vote, a sentiment similar to what many perceived as the president's hypocritical pronouncement that the Senate's 85 to 8 vote on June 15, 1982, was a "statesmanlike decision" (pp. 291, 292). Lawson richly re-creates and then analytically takes apart the shifting coalitions, elaborate lobbying, incremental impact of new black voters, White House strategies under Nixon and Reagan to obfuscate or gut the law (the banning of northern literacy tests, the discovery of discriminatory intent, and so on), and repeated, bipartisan defense of protected minority access to the ballot (Republican senator Robert Dole "was superb," said civil rights attorney Joe Rauh enthusiastically. "We couldn't have done it without him" [p. 291]).

No miracles have been wrought by the much-strengthened minority franchise. As Lawson observes, Afro-American voter turnout in the seven states of the Deep South continues to disappoint; only about 46 percent of the 58 percent eligible voters participated in presidential elections since 1976. Even though Afro-American elected officials now number almost two thousand in these same seven states (a truly startling jump from the zero figure of twenty years ago), they represent a mere 5 percent of the total number of elected officials. Lawson is even more troubled by the failure of Afro-Americans to elect a single representative of their race in the eighty counties of the Deep South in which they comprise 50 percent of the population. Moreover, in the Deep South as a whole the annual rate of increase of Afro-American elected officials has plunged from a high of 27 percent (1970) to 8 percent (1981). Finally, the federal courts seem increasingly indisposed to favor minority challenges to at-large candidacies. Nationwide, Afro-American elected officials account for only 1 percent of the total.

The caveat of one scholar consulted by Lawson about the limited efficacy of even the most carefully

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drafted voter legislation reflects a cynicism born of compounded frustrations: "It is perfectly possible to have people voting all day long, and the vote to be relatively useless, because of the way the rules of the game are rigged against them" (p. 217). But it is Charles Evers of Mississippi to whom the civil rights historian must turn for the higher perspective on all this. "C'mon now," Evers exhorted his followers. "This ain't civil rights no more. Y'all got your local candidates. They gotta buy placards, bumper stickers—that stuff ain't free. Prayer and marchin' ain't gonna get elected officials" (p. 263). Another profoundly significant point Lawson's book makes is that, even after the stickers and placards have been purchased and the minority candidates elected, the Second Reconstruction will have "failed to remove the serious economic impediments hampering racial equality. Many southern blacks today face as bleak an economic future as their ancestors of a century ago" (p. 302).

DAVID LEVERING LEWIS
Rutgers University

DENNIS LAURENCE CUDDY, editor. *Contemporary American Immigration: Interpretive Essays.* Volume 1, *European*; volume 2, *Non-European.* (Immigrant Heritage of America.) Boston: Twayne. 1982. Pp. 223; 267.

Contemporary American Immigration is a two-volume collection of essays on various groups of people who have entered the United States since 1945. Volume 1 focuses on Europeans (Italians, Irish, Poles, Norwegians, Germans, Dutch, Portuguese, Lithuanians, and British); volume 2 deals with almost everyone else except South Americans (Asians, Mexicans, Cubans, blacks, Puerto Ricans, Canadians, Arabs, and Australians). These essays, written about 1978 and published four years later, are purportedly an interpretive analysis of recent immigration, but the individual pieces follow no uniform format (although one was given to the individual authors). Some of the essays roam back to the colonial era; others deal almost exclusively with people already in this country before World War II and their descendants. The quality, in general, ranges from acceptable to mediocre. Scholars will learn nothing about the groups with which they are already familiar; others might garner an occasional insight or obtain a much-needed statistic.

Perhaps when Dennis Laurence Cuddy had the idea for this project, not enough monographs had been written on post-1945 immigrants to get syntheses of superior quality. But certainly one must go beyond the broadest generalizations about who came for what reasons, where they settled, and what kind of work they did. The added statistics are useful but not particularly revealing. There is little interpretation in these essays aside from discussions of generational conflict within the families and difficulties for all of the newcomers in adjusting to American culture.

Some of the articles are better than others. Robert Billigmeier's discussion of the Germans is one of the more fully detailed and analyzed pieces in the book. *Reichsdeutsche* and *Volksdeutsche* are distinguished and explained; emigration and occupational statistics are broad and carefully broken down; a map clearly shows how many Germans went to which American communities; and the diversity of German groups is elaborated on within the demands of tight spacial limitations. H. Brett Melendy's combined study of the Filipinos, Koreans, and Vietnamese, Joseph Fitzpatrick's compression of a great deal of information on the Puerto Ricans into one essay, and Odd S. Lovoll's selection on the Norwegians are also worthwhile.

Almost every major group of immigrants except European Jews is covered. The editor explains this omission in the preface to his non-European volume. The author who was to have written about the Jews became ill, and time did not allow for a replacement to fulfill the commitment. I was taken aback, however, to read a full essay on Lithuanian displaced persons, who, author Algirdas M. Budrechis claims, are "a tragic reminder of the uprooting and convulsive displacement of whole populations as a result of total war and totalitarian dictatorship" (vol. 1, p. 173). In all frankness, I must admit that, when I think of the devastation wrought by Hitler during and after World War II, Lithuanians are not the first group that comes to mind. Nevertheless, the essay is informative about a people that fled the Russians, spent time in Germany, and entered the displaced persons camps after the war before finding permanent homes elsewhere in the late 1940s and early 1950s.

It would be difficult to suggest an appropriate audience for these volumes. The research and analyses are much too thin to benefit scholars; the articles are generally without human interest and inadvisable for generally intelligent and informed readers; and they are, for the most part, insufficiently detailed or insightful to help students who want to learn something about a particular group.

Why were these books published? My guess is that the editor and publisher hoped to capture the interest of those anxious to read something about post-1945 immigration. To that end the collection succeeds. The essays are something, but they are not much more than that.

LEONARD DINNERSTEIN
University of Arizona

SILVIA PEDRAZA-BAILEY. *Political and Economic Migrants in America: Cubans and Mexicans.* Austin: University of Texas Press. 1985. Pp. vii, 242. $27.00.

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The large and recent influx of Central Americans into the United States has raised the issue of what distinguishes a political refugee from an economic refugee. Silvia Pedraza-Bailey reminds us that this issue, as it pertains to Latin American conditions, is not a new one. Examining Cuban and Mexican immigrants since 1945, she concludes that the distinction is based on two major factors. One, of course, concerns the reasons why people decide to leave their homeland. The other revolves around the reception that the state, in this case the U.S. federal government, provides the refugees. Taking these two variables into account, she examines the question of why Cubans have become a "success story" while Mexicans are considered a "silent invasion."

Pedraza-Bailey anchors her thesis on the concept of structural assimilation, that is, the economic and educational assimilation of immigrant populations. Using comparative data, she documents what is popularly known—that Cuban immigrants have fared considerably better than Mexican immigrants in levels of income, education, and employment. Pedraza-Bailey concludes that the state has exerted a major influence in determining how these two ethnic groups have been received in the United States. Mexicans have been useful and profitable for this country as cheap surplus labor. Consequently, they have been treated as economic refugees, and the state has not felt it necessary to provide them with assistance programs that would accelerate their integration. By contrast, Cuban refugees, certainly since the 1959 revolution, have served a distinct political-propaganda purpose for the state; hence, they have not only been given sanctuary but have been accorded special privileges such as job training and college financial aid. Pedraza-Bailey contends that the state's reception of immigrants is an integral variable that has been missing in theories of assimilation.

Although Pedraza-Bailey makes an effort at providing a historical framework, her study is essentially a sociological one. The historical context suffers from a failure to use the best of Chicano historiography. Also troubling is her uncritical use of the term "silent invasion" to categorize Mexican immigration. Such terminology is more appropriate in the "yellow press" than in a scholarly monograph. Pedraza-Bailey more effectively analyzes Cuban refugees, whom she correctly portrays as a heterogeneous population. When she turns to Mexicans, however, she inappropriately portrays them as a homogeneous mass, even though Mexicans have also emigrated as political refugees, especially during the revolution of 1910. Finally, the study would have been better served if Pedraza-Bailey had prevented her own anticommunist and anti-Castro biases from surfacing in what is otherwise a useful comparative study.

MARIO T. GARCIA
University of California, Santa Barbara

CANADA

G. A. RAWLYK. *Ravished by the Spirit: Religious Revivals, Baptists, and Henry Alline.* (Hayward Lectures, 1983.) Kingston: McGill-Queen's University Press. 1984. Pp. xiv, 176. Cloth $19.95, paper $6.95.

Between 1775 and 1784 Henry Alline, a tanner turned evangelical preacher, initiated and inspired a powerful religious "awakening" in the scattered settlements of Nova Scotia and died in New Hampshire in 1784 at the outset of a mission to New England. For many of his contemporaries he was "a man almost larger than life" (p. vii). In a letter written to William Black on July 13, 1783, John Wesley, however, asserted that "Mr. Alline may have wit enough to do hurt; but I fear he will never have wit enough to do good." A century later, the memory of Alline's charismatic preaching had faded, except among the Baptists in New Brunswick. Recently, however, some Canadian historians have concluded that the evangelical tradition in Canada is "deserving of scholarly attention" (p. 111), and the short but dramatic career of Henry Alline, one of the founders of that tradition, has become one of the focal points of this investigation. The writings of G. A. Rawlyk on Alline and the Baptists in the Maritime Provinces are a major component of this growing body of historical literature.

Ravished by the Spirit consists of four lectures delivered at Acadia University. In these, the author has outlined the evolution of his own understanding of Alline and his work, assessed the nature and extent of Alline's influence in Nova Scotia and in New England, and has raised some important questions for further research.

The first lecture, "Alline's Alline," is a fascinating sketch of a poorly educated, morbidly introspective product of the New England Calvinist world, who in his own words was "ravished" by a "divine ecstasy" (p. 5) and inspired his followers to believe that all could experience the same rapture. His mystical, anti-Calvinist conviction that conversion is "marriage to Christ" (p. 15) was the product of his own inner experience and proved immensely appealing to his fellow Nova Scotians.

On the basis of the evidence cited by Rawlyk it would be easy to explain Alline's teaching and career in the secular language of psychology and as a response to the social and political conditions of

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Nova Scotia. The significant point is that he was heard and judged in the context of an evangelical climate of opinion based on a belief in a divine reality capable of breaking into the human world. So powerful was this conviction that Alline's message was heeded by thousands in Nova Scotia and helped generate a revival in northern New England. His heterodox doctrines fostered an outbreak of antinomianism and a liberal attitude toward doctrine, polity, and liturgy. The Baptist community in Nova Scotia, founded largely by former disciples of Alline, took shape in reaction to these unmanageable pressures. But, Rawlyk emphasizes, they upheld his view that "personal salvation should be man's only concern" and thus discouraged the development of an "evangelical social ethic" (pp. 104–05).

Rawlyk's scholarly but sympathetic account of Alline and his legacy should stimulate renewed interest in the relationship between the evangelical past of Nova Scotia and its present condition. Above all, this brief study reminds us that the comparative study of the role of evangelical religion in the United States and Canada is still in its infancy. Rawlyk has deepened our understanding of Alline and has pointed the way to this other vast territory.

GOLDWIN FRENCH
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Toronto, Ontario

T. W. ACHESON. *Saint John: The Making of a Colonial Urban Community*. Toronto: University of Toronto Press. 1985. Pp. 314. $29.95.

T. W. Acheson's St. John hums to the rhythms of colonial and American life. Distinctions among freeholders, freemen, nonfree British subjects, aliens, blacks, and business women had legal status. An Established Church existed alongside many other denominations and sects. A merchant elite, large numbers of artisans, and poor laborers inhabited its houses. Native and newcomer alike made their homes here, the former often of loyalist stock who had migrated from the new American republic and the latter most probably Irish.

St. John was an important place in the pre-Confederation North American colonies. Acheson examines its public life and private interests during a period when it grew from insignificance to become the fourth largest colonial city. By 1851 only Montreal, Quebec, and Toronto surpassed it in population. Its decline, which was already obvious from the figures in the 1871 census, followed precipitously. Acheson's story, however, concludes with the apogee of mercantile and artisanal achievement, offering only some hints of the years of rapid decline. Colonial St. John rose with the timber trade and the market for wooden sailing vessels. This prosperity was founded on a staple the supply of which was determined by the resources of the hinterland and the demand for which was set in distant metropolises. It was an inherently unstable foundation on which to build, as is evident even in the decades during which St. John grew as her merchants and artisans prospered. Crises there were aplenty before 1860, and the political history recounted here is organized around them because final recourse was sought within the framework of imperial relations. Here Acheson gives little attention to the larger context of nation building and only slight recognition of the presence of the United States a few miles away. St. John, caught within intricate webs of imperial and international economic, social, and political relationships, was nevertheless isolated from all its neighbors.

For the reader who has had a surfeit of structural analyses under the banner of urban history in recent years, this book, devoted to so careful and detailed an exploration of local circumstances and meanings, will be welcome. Retrieving the calculus of contemporary social and economic distinctions in a colonial urban community, it offers an intimate acquaintance with the place. This it does with care and with fervor.

PETER G. GOHEEN
Queen's University
Kingston, Ontario

JAMES G. SNELL and FREDERICK VAUGHAN. *The Supreme Court of Canada: History of the Institution*. Toronto: University of Toronto Press, for the Osgoode Society. 1985. Pp. xv, 319. Cloth $40.00, paper $17.50.

A few pioneering books offer sudden, breath-taking perspectives like Balboa's at Darien; others, the majority, traverse a more mundane landscape, taking one through the forest on a road newly cut, the stumps still naked on either side and the track itself occasional hard going. The marvel is that the road is there at all. One can see the work that went into this book by James G. Snell and Frederick Vaughan, which has 50 pages of notes for 250 pages of text. One also gets a valuable, pioneering bibliography! It is a book, in short, hewn from the sources.

The constitution of 1867 envisaged Canada *a mari usque ad mare*, or, as it ought more properly to be (including the Arctic with Atlantic and Pacific) *a mari usque ad maria*. The constitution of 1867 allowed for a supreme court, but for the first eight years of Canada's existence none was established. The authors seem puzzled about why Sir John A. Macdonald, who was largely responsible for the 1867 constitution, should have hesitated over the establishment of a supreme court. The reason was

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that French Canadians feared it. As Henriette Chapais wrote to her son, "[Since 1867] Sir John Macdonald had twice wanted to introduce this bill, but his colleagues from Lower Canada—George Cartier, your father, and [Hector] Langevin—had always prevented him from doing it . . . especially because, of the seven judges to be nominated, they could only put forward two judges from Lower Canada" (*Mémoires Chapais*, vol. 3, p. 33).

When the Supreme Court was finally established in 1875—under another prime minister—it raised the question of whether appeals could still be made to the Judicial Committee of Privy Council in London, the supreme law authority of the empire. Ultimately, appeals were allowed; those from provincial courts were even allowed to by-pass the Supreme Court of Canada. The court was neither master in its own country nor master of its own decisions. *Stare decisis* made it derivative. This was the more serious since the Privy Council tended to exalt the provinces above the federal government and, in a series of decisions from 1883 to 1930, proceeded to undo the centralizing intentions of the founding fathers of 1867.

So the Supreme Court of Canada had to struggle to maintain an identity; a niggardly government housed it indifferently, paid its justices a pittance, and appointed them out of a mixture of concern for the law and concern for the political affiliation of the candidate. That the court succeeded as much as it did is surprising. At last, in 1949, with a sense of what belonged to the standing of the country, overseas appeals were ended. Since 1982, when a charter of fundamental rights gave it a large and serious role, the Supreme Court of Canada has had the challenge of new and vigorous responsibilities.

This is a handsome book with which the authors can be pleased. It is too bad that so much judicial gossip has been excised, and one does object to modern illiteracies such as "substantive" when "substantial" is meant. The index is thin when it needs to be generous. But let critics cavil; solid and useful books like this will survive and flourish.

P. B. WAITE
Dalhousie University

DAVID J. BERCUSON. *Canada and the Birth of Israel: A Study in Canadian Foreign Policy*. Toronto: University of Toronto Press. 1985. Pp. xi, 291. $24.95.

Poor Canada! One by one her national myths are being shattered. No longer can Canadians sit smugly in judgment of others without taking into account their own record. And that record, as recent historical works painfully point out, was not of an open, hospitable, liberal society, nor of a nation of foreigners in which racism was itself foreign, as Canadians were brought up to believe. Rather, these new studies present a Canada almost unrecognizable to Canadians—a Canada whose treatment of its native population was as callous as that of our neighbors to the south, a Canada whose record in providing sanctuary to Jews fleeing Nazi persecution was the worst in the Western world, a nation that was brutal and heartless in its treatment of its Chinese and Japanese immigrants, a society whose record on human rights and civil liberties was appalling.

But one myth remained: that of the indomitable Canadian public servant in the golden years of the 1930s and 1940s. These were the brilliant, urbane men—and almost all of them were men—recruited from the academic world to Ottawa who supposedly were putting Canada on the map and influencing world events. These men were model civil servants: progressive, creative, and just. Now even this myth is under assault.

In this significant book David J. Bercuson paints a largely unattractive portrait of Canadian diplomats in the 1940s, the so-called high point of Canadian diplomacy. In doing so, he also lays to rest another enduring myth. It is commonplace for Canadians to regard their country as serving as a midwife to the birth of Israel and as being a bulwark of support for the Jewish state. The truth, however, as Bercuson makes clear, is far different.

There was little support in Ottawa for the Zionist cause. Indeed, Canada wanted desperately to be left alone. Officials in the Office of External Affairs were beside themselves trying to offend no one, neither their one-time colonial masters in London nor their new continental masters in Washington. In the end, of course, their pusillanimous behavior pleased neither country and offended both the Israelis and the Arabs. Canada was the last of the Dominions to recognize Israel—and did so only grudgingly at the end of 1948.

Based on extensive research in archives in Canada, Great Britain, Israel, and the United States as well as on interviews with some of the major actors, the book is the definitive study of a major turning point in Canadian diplomatic history.

Bercuson's thesis is a significant one. For too long Canadians and others believed that the policies of the Canadian government toward the Middle East were influenced both by the "Jewish lobby" and by a haunting sense of guilt over the Holocaust. This book proves beyond any doubt that the so-called Jewish lobby was ineffectual and that as for guilt over the Holocaust—despite Canada's complicity by closing her doors to Hitler's victims—there was none. Canadian foreign policy in the Middle East, as in other areas, was based solely on Canadian interest as perceived by officials in Ottawa, nothing more, nothing less.

IRVING ABELLA
York University

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LATIN AMERICA

JEAN STUBBS. *Tobacco on the Periphery: A Case Study in Cuban Labour History, 1860–1958*. (Cambridge Latin American Studies, number 51.) New York: Cambridge University Press. 1985. Pp. xiii, 203. $42.50.

Tobacco was always Cuba's other industry: although never enjoying quite the same political influence or economic importance as sugar, it nevertheless was a significant source of foreign exchange and domestic prosperity. At its height during the nineteenth century, tobacco provided employment for more than one hundred thousand Cubans, in cultivation of the leaf and in manufacture of the cigar, in Cuba as well as abroad. The workers formed an important sector of the Cuban proletariat. In the course of the next century the influence of cigar workers in Cuban national life was well out of proportion to their numbers. They figured prominently in the nineteenth-century struggles for independence, were decisive in the formation of the National Confederation of Cuban Workers, and participated in the founding of the Cuban Communist party. They were, in short, a presence in all of the revolutionary upheavals of late nineteenth- and twentieth-century Cuba.

This is a familiar story in Cuba. It is not, however, well known outside the island, and for this reason the appearance of Jean Stubb's work is a most welcome contribution to the scholarship on Cuba. This slim volume goes a long way toward closing one of the more conspicuous gaps in the English-language literature and, in so doing, provides a model of careful research and judicious scholarship.

The study is arranged in three distinct and unequal parts. A brief introductory section surveys the tobacco and cigar-making industry from the early nineteenth century to the triumph of the Cuban revolution in 1958. The middle part of the work, the longest section, examines the vital issues of the Cuban labor movement: distinctions of class, gender, and race. The concluding epilogue provides a brief assessment of tobacco in socialist Cuba.

The first and last parts deal expertly with the origins and development of the industry before and after the revolution. It is a straightforwrd narrative, written in not especially inspired but at least informative prose. Its purpose is, in fact, to set the context for the middle section, which is where the study blossoms. Based on a vast array of archival materials, oral histories, and unpublished industry and union records, Stubbs's work is a sensitive analysis of tobacco workers, not only the peasants and farmers who cultivated the leaf in fields but also the craftspeople who rolled the cigars in the factories. Stubbs deftly integrates the account of the cigar makers into the larger setting of the workers' struggle and revolutionary politics during the late colony and the early republic. This is more, therefore, than a history of an industry or a trade union. The real achievement is that Stubbs places her subject in a full historical setting and, in so doing, provides an important insight into the very development of *Cubanidad*. Stubbs has made an important contribution to our understanding of the Cuban labor movement, the role of the cigar workers within that movement, and the part played by tobacco in the formation of Cuban nationality. The book will serve as a standard reference work on the subject for years to come.

LOUIS A. PÉREZ, JR.
University of South Florida

JAMES DUNKERLEY. *Rebellion in the Veins: Political Struggle in Bolivia, 1952–82*. London: Verso. 1984. Pp. xvi, 385. Cloth $30.00, paper $9.50.

The three decades covered in this survey were among the most complex, bloody, and intense political periods in Bolivian history. They include the major political, economic, and social transformations brought about by the National Revolution of 1952, the wildly variant populist military-peasant regimes of the post-1964 period, the seemingly new model authoritarian regime of the OPEC boom period of the 1970s, and the beginnings of the long, bitter struggle to re-create a popular participatory democratic society after 1978. James Dunkerley, who wrote an interesting dissertation on the Bolivian army in the period 1879–1935, knows the sources and politics of Bolivia well and has a good understanding of the basic issues. He undertakes both a close reading of the major secondary literature and a reasonable sampling of primary sources such as periodicals and pamphlets. The result is an interesting interpretive essay with many original materials and insights.

Despite its usefulness, however, the book does have some problems. On the whole, Dunkerley develops a sophisticated and often-original analysis of the political machinations and alliances of the period, but sometimes he allows himself a flippant style, what his publisher calls approvingly his "literary panache." This style often leads the author into simplistic generalizations and unintended paternalism. At his best in dealing with organized labor, he is less sure in his understanding of economic and social structures. This is evident in his rather mechanistic interpretation of the traditional elite prior to 1952 and the role of the United States afterward, just as it is in his comments on population policy and settlement patterns. Although he offers original materials on the role of the peasants in the period of agrarian unrest in 1952–53, he is insensitive to their political evolution in the period under study, and he

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has little understanding of the dynamics of the *military-peasant alliance regimes* of 1964–74. His dependence on the writings of Guillermo Lora, an important historian but a biased political participant, makes Dunkerley exaggerate the role of the Trotskyite parties and underplay the importance of all other Marxist and nationalist groups. Finally, given the violently repressive regimes of the last decades in Latin America, it is amazing to read Dunkerley's assessment of the unique civil disobedience campaign that brought down the dictatorship of General Hugo Banzer as one in which "the importance of elementary democratic liberties served to dazzle the working classes and peasantry" (p. 231).

But, when it comes to understanding the shifting alliances and political machinations of army leaders, middle-class reformers, and radicalized workers, there is no better introduction to the subject. Several partial studies on this period have appeared, but Dunkerley's book is the first full-scale survey and the best introduction to date on modern Bolivian politics.

HERBERT S. KLEIN
Columbia University

R. A. HUMPHREYS. *Latin America and the Second World War.* Volume 1, *1939–1942.* (University of London, Institute of Latin American Studies, number 10.) London: Athlone; distributed by Humanities Press, Atlantic Highlands, N.J. 1981. Pp. 232. $42.50.

R. A. HUMPHREYS. *Latin America and the Second World War.* Volume 2, *1942–1945.* (University of London, Institute of Latin American Studies, number 11.) London: Athlone; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. 296. $38.00.

This long-awaited work by R. A. Humphreys is a carefully researched, smoothly written study. These volumes, which will serve as a basic reference work for students of Latin American affairs, are indispensable reading for those who want to understand the relations between the United States and the nations of Latin America and for those who wish to enter into a more profound analysis of the complex dilemma that confronts the nations of Latin America today.

The books' organization is straightforward; major chronological periods in the immediate prewar years constitute the chapters of the first volume, and the war itself is the chronological framework of the second volume. Within each major period, Humphreys provides skillful summaries of each country in turn. The author provides more detail for the larger countries—Mexico, Brazil, and Argentina—and concentrates on Argentina's policy of neutrality during the war. The broad introduction to Latin America in the first volume effectively sets the stage for what follows. And the chapter on the postwar world at the end of the second volume effectively sets Latin America and inter-American relations into a global context.

The research that has gone into these volumes is one of the study's strengths. Humphreys has read widely in the secondary literature on Latin America and on U.S. foreign policy, and he has studied the documents in the British Public Record Office (PRO) more intensively and over a longer period than anyone else in the field. This comprehensive mining of the Foreign Office records is both a virtue and a vice, for the author provides a brilliant summary of a complex set of events from the British perspective but claims to write a history that balances the British, U.S., and Latin American points of view. Hence, events are portrayed on the basis of a profound and detailed knowledge of official British records and a rather superficial harvest of information from published U.S. sources. The level of information about Latin American governments and their policies is even less profound, for the author depends on a rather narrow range of monographic studies and the British documents for his statements about Latin American policies and actions.

The British perspective leads the author to a number of interesting, provocative assertions. For example, he points out the increase of Latin American dependence on the United States during the era of neutrality. He is properly skeptical about U.S. claims of cooperating with Latin America during this period and insists that the United States did not have the goods to supply Latin America. Ironically, this skepticism concerning the capacity of the United States to pick up the slack created by British decline does not carry over into his treatment of the network of buying commissions created by the United States to guarantee the Allies the primary products they needed to prosecute the war against the Axis. Here his tone is traditional and ethnocentric, reminding me of the textbooks of Samuel F. Bemis.

The treatment of Mexico's experiences during the war is especially interesting. Humphreys succeeds in explaining how Mexico's proximity to the United States affected its foreign policy and how the successful conclusion of the oil expropriation crisis made it possible for the two nations to cooperate during the war. As a sidelight on the central story, he notes that Mexican laborers went to the United States during the war to relieve labor shortages caused by the draft, thereby setting the stage for the later problem of illegal immigration.

The history of Argentina's involvement in hemispheric affairs during the period receives the greatest attention, as well it might since Argentine neu-

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trality was the most irritating episode in hemispheric relations during the war. But the author's handling of the episode is probably the clearest example of how his reliance on PRO documents skews his narrative. Although I found his explanation of Argentine neutrality as convincing as any in English, it seems silly to spend so much time rearguing the debate between Cordell Hull and Sumner Welles on the one hand and the British officials on the other about whether the British position on the Argentine question was an attack on the United States or support for a reasonable Argentine position. He refers to the American allegations as nonrational; he says the British hostility to which they referred was a myth. At another point he writes, "The Foreign Office, at once more cautious and more skeptical in its interpretation of what had happened than the State Department" (vol. 2, p. 167). If Humphreys had consulted the State Department records, he would have found excellent reports from the field, every bit as comprehensive and cautious as the British reports. The fact that Hull chose to ignore those reports is another matter, one that leads to an analysis of the way in which policy is formulated in the U.S.

But I do not want to end this review on a negative note. I want to emphasize that this is an extremely valuable contribution to our knowledge of hemispheric affairs and will serve for many years to come as the best summary of events during the wartime period.

JOSEPH S. TULCHIN
University of North Carolina

JOHN D. WIRTH, editor. *Latin American Oil Companies and the Politics of Energy*. (Latin American Studies.) Lincoln: University of Nebraska Press. 1985. Pp. xxxix, 282.

Latin American governments and their plethora of big-spending public enterprises are often blamed for contemporary bouts with inflation and indebtedness. Budget-cutting seldom succeeds, however, even when attempted by powerful military juntas. Many things account for this failure, but most prominent among them is the resistence of civilian and military interests that have benefited from such enterprises since their creation. How this came to be is described in a series of case studies presented in this book about government oil companies edited by John D. Wirth.

Developed from papers presented at the American Historical Association meetings in 1981, the book describes efforts to create and operate government petroleum industries in Argentina, Brazil, Mexico, and Venezuela during the first half of the twentieth century, the only exception being Venezuela where the government did not nationalize the industry until 1976. Regrettably, no attempt is made to compare the various national experiences and explore the different consequences of swift (Mexico) and gradual (Argentina) enterprise development, early (Mexico) and late (Venezuela) nationalization, or the differences between an export industry (Mexico and Venezuela) and one confined by modest supplies to production entirely for domestic markets (Brazil and Argentina).

A few similarities and differences are obvious to the reader. The four nations were driven less by socialist ideology than by a nationalism that was on the rise throughout the region after World War I. State capitalism was an exciting, if clumsy, response to popular demands for national exploitation of natural resources in countries whose public philosophies retained Hispanic notions of state paternalism. This came at a time when it was politically popular to question the propriety of foreign penetration. How governments built their industries varied substantially, however. As the late Carl Solberg shows in his excellent essay on the origins of Argentina's *Yacimeintos Petroleos Fiscales* (YPF), the nationalist consensus that formed swiftly in Mexico during the 1930s eluded Argentina, where an economic oligarchy that had built its power on an export economy resisted offending its trading partners by allowing nationalists to succeed. Moreover, after the YPF was established, the high-consuming Argentines could not afford to allow as much government ownership of industry as the Mexicans had but instead had to seek the help of multinational firms repeatedly.

Clearly, we can learn much from case studies that document the origins of state capitalism in Latin America. But more comparative history must be written if we are to understand the forces that were most influential in its development.

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Carleton College

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

O. M. BRACK, JR., editor. *Studies in Eighteenth-Century Culture*. Volume 15. Madison: University of Wisconsin Press, for the American Society for Eighteenth-Century Studies. 1986. Pp. xi, 295. $25.00.

O. M. BRACK, JR., Preface. JEAN A. PERKINS, Presidential Address: The First Fifteen Years of the American Society for Eighteenth-Century Studies. ARAM VARTANIAN, Diderot and the Technology of Life. KATHRYN SIMPSON VIDAL, Diderot and Reader-Response Criticism: The Case of *Jacques le fataliste*. THOMAS M. CARR, JR., Voltaire's Fables of Discretion: The Conte philosophique in *Le Taureau blanc*. REED BENHAMOU, Continuing Education and Other Innovations: An Eighteenth-Century Case Study. FRANK BAASNER, The Changing Meaning of "Sensibilité": 1654 till 1704. MICHELLE BUCHANAN, Savages, Noble and Otherwise, and the French Enlightenment. MONICA LETZRING, Teresa Margarida da Silva e Orta and the Portuguese Enlightenment. PETER PETSCHAUER, Christina Dorothea Leporin (Erxleben), Sophia (Gutermann) von La Roche, and Angelika Kauffmann: Background and Dilemmas of Independence. HENRY L. FULTON, A Scottish Middle-Class Family and Patronage: The Ancestors of Sir John Moore. CHARLES H. HINNANT, Dryden's Definition of a Play in *An Essay of Dramatic Poesy*: A Structuralist Approach. J. M. ARMISTEAD, Calista and the "Equal Empire" of Her "Scared Sex." MANUEL SCHONHORN, Defoe, Political Parties, and the Monarch. DAN DOLL, The Word and the Thing in Swift's Prose. MARK W. BOOTH, Song Form and the Mind in Christopher Smart's Later Poetry. THOMAS JEMIELITY, Samuel Johnson, *The Vanity of Human Wishes*, and Bibliographical Criticism. JOHN A. VANCE, Johnson and Hume: Of Like Historical Minds. SAMUEL H. WOODS, JR. Images of the Orient: Goldsmith and the Philosophes. GEORGE B. WALL, Is God Really Omniscient? Or What Does God Know about Pain? SYLVIA H. MEYERS, Learning, Virtue, and the Term "Bluestocking."

DAVID CLOSE and CARL BRIDGE, editors. *Revolution: A History of the Idea*. Totowa, N.J.: Barnes and Noble. 1985. Pp. 230. $23.50.

DAVID CLOSE, The Meaning of Revolution. P. A. HOWELL, The Greek Experience and Aristotle's Analysis of Revolution. P. A. HOWELL, The Transition from Classical to Modern Times. P. A. HOWELL, Liberal Thought about Revolution from Locke to Mazzini. CARL BRIDGE, Burke and the Conservative Tradition. NORMAN WINTHROP, Marx, Lenin, and Modern Revolutions. BILL BRUGGER and KATE HANNAN, Modernization and Revolution. DAVID CLOSE, The Soviet Union: A Defunct Revolution. BILL BRUGGER, China: An Interrupted Revolution. DAVID CLOSE, Nazi Germany: A Doomed Revolution. CHERRY GERTZEL, Guinea-Bissau: Revolution and Development. RICHARD DEANGELIS, France, May 1968: A New Kind of Revolution?

DONALD MACKENZIE and JUDY WAJCMAN, editors. *The Social Shaping of Technology: How the Refrigerator Got Its Hum*. Philadelphia: Open University Press. 1985. Pp. 327. $17.00.

LANGDON WINNER, Do Artifacts Have Politics? THOMAS P. HUGHES, Edison and Electric Light. RUTH SCHWARTZ COWAN, Gender and Technological Change. CYNTHIA COCKBURN, Caught in the Wheels: The High Cost of Being a Female Cog in the Male Machinery of Engineering. MARC BLOCH, The Watermill and Feudal Authority. KARL MARX, The Machine versus the Worker. HARRY BRAVERMAN, Technology and Capitalist Control. TINE BRULAND, Industrial Conflict as a Source of Technical Innovation: The Development of the Automatic Spinning Mule. WILLIAM LAZONICK, The Self-Acting Mule and Social Relations in the Workplace. DAVID F. NOBLE, Social Choice in Machine Design: The Case of Automatically Controlled Machine Tools. CYNTHIA COCKBURN, The Material of Male Power. JANE BARKER and HAZEL DOWNING, Word Processing and the Transformation of Patriarchal Relations of Control in the Office. MIKE COOLEY, Drawing Up the Corporate Plan at Lucas Aerospace. RUTH SCHWARTZ COWAN, The Industrial Revolution in the Home. RUTH SCHWARTZ COWAN, How the Refrigerator Got Its Hum. MOYRA DOORLY, A Woman's Place: Dolores Hayden on the "Grand Domestic Revolution." WILLIAM H. MCNEILL, De Gribeauval and the Origins of Planned Invention for War. JAMES FALLOWS, The American Army and the M-16 Rifle. MICHAEL H. ARMACOST, The Thor-Jupiter Controversy. MARY KALDOR, The Armament Process. MARY KALDOR, Military Technology in the Soviet Union. ALAN ROBERTS, Preparing to Fight a Nuclear War.

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ISABEL MCBRYDE, editor. *Who Owns the Past?* (Papers from the Annual Symposium of the Australian Academy of the Humanities.) New York: Oxford University Press. 1985. Pp. 198. $32.50.

ISABEL MCBRYDE, Introduction. BRUCE TRIGGER, The Past as Power: Anthropology and the North American Indian. ERIC WILLMOT, The Dragon Principle. LES GROUBE, The Ownership of Diversity: The Problem of Establishing a National History in a Land of Nine Hundred Ethnic Groups. BERNARD SMITH, Art Objects and Historical Usage. JOHN MULVANEY, A Question of Values: Museums and Cultural Property. DAVID WILSON, Return and Restitution: A Museum Perspective. ALICE ERH SOON TAY, Law and the Cultural Heritage. SHARON SULLIVAN, The Custodianship of Aboriginal Sites in Southeastern Australia. ELIZABETH JEFFREYS, Byzantium: The Transmission of a Heritage. WANG GUNGWU, Loving the Ancient in China.

PATRICIA J. F. ROSOF and WILLIAM ZEISEL, editors. *Family History*. Reprint. New York: Institute for Research in History or Haworth, New York. 1985. Pp. 171. $22.95.

DOROTHY O. HELLY, Introduction: The History of the Family and the History of Family History. JON-CHRISTIAN BILLINGMEIER, Studies on the Family in the Aegean Bronze Age and in Homer. SARAH B. POMEROY, The Family in Classical and Hellenistic Greece. SUZANNE F. WEMPLE, The Medieval Family: European and North American Research Directions. LOUISE A. TILLY, Demographic History Faces the Family: Europe since 1500. LAUREL THATCHER ULRICH, The Family History of Early America. MICHAEL DAHLIN, The U.S. Family in the Industrial and Post-Industrial Periods. HARRIETTE PIPES MCADOO and ROSALYN TERBORG-PENN, Historical Trends and Perspectives of Afro-American Families. DIANA BALMORI, Trends in Latin American Family History. KAREN TRANBERG HANSEN and MARGARET STROBEL, Family History in Africa. PATRICIA EBREY, Family and Kinship in Chinese History.

ALEX ROLAND, editor. *A Spacefaring People: Perspectives on Early Spaceflight*. (NASA History Series.) Washington, D.C.: Government Printing Office. 1985. Pp. viii, 156.

J. A. SIMPSON, Space Science and Exploration: A Historical Perspective. PAMELA MACK, Satellites and Politics: Weather, Communications, and Earth Resources. ARNOLD S. LEVINE, Management of Large-Scale Technology. I. B. HOLLEY, JR., Commentary. RICHARD P. HALLION, The Next Assignment: The State of the Literature on Space. JOHN NOBLE WILFORD, A Spacefaring People: Keynote Address. SYLVIA DOUGHTY FRIES, Commentary. JOHN LOGSDON, Opportunities for Policy Historians: The Evolution of the U.S. Civilian Space Program. WALTER A. MCDOUGALL, Space-Age Europe, 1957–1980. EDWARD C. EZELL, Space Activities in the Soviet Union, Japan, and the People's Republic of China. RICHARD S. KIRKENDALL, Commentary. BRUCE MAZLISH, The Idea of Space Exploration. JAMES A. MICHENER, A Humanist's View. CARROLL W. PURSELL, JR., Commentary.

QUENTIN SKINNER, editor. *The Return of Grand Theory in the Human Sciences*. New York: Cambridge University Press. 1985. Pp. viii, 215. Cloth $29.50, paper $8.95.

QUENTIN SKINNER, Introduction: The Return of Grand Theory. WILLIAM OUTHWAITE, Hans-Georg Gadamer. DAVID HOY, Jacques Derrida. MARK PHILIP, Michel Foucault. BARRY BARNES, Thomas Kuhn. ALAN RYAN, John Rawles. ANTHONY GIDDENS, Jürgen Habermas. SUSAN JAMES, Louis Althusser. JAMES BOON, Claude Lévi-Strauss. STUART CLARK, The *Annales* Historians.

E. P. THOMPSON, editor. *Star Wars*. New York: Pantheon. 1985. Pp. 165. $5.95.

DOROTHY HODGKIN, Foreword. E. P. THOMPSON, Why Is Star Wars? BEN THOMPSON, What Is Star Wars? JOHN PIKE, Assessing the Soviet ABM Programme. RIP BULKELEY, The Effects of SDI on Disarmament. E. P. THOMPSON, Folly's Comet.

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REVIEWS OF BOOKS

TO THE EDITOR:

Zvi Razi's review of my *Control of Late Ancient and Medieval Population* (*AHR*, 91 [1986]: 369), fails to mention much of the subject matter in the book and depreciates unduly the excellent work of the physical anthropologists who produced the nearly five hundred reports of excavations of cemeteries on which much of the book is based.

The first chapter indicates that the population of imperial Rome was about 150–200,000 and thus hardly a mass of urban slums. The fourth chapter gives the data on expectation of life at several ages, showing that, although the general average was low, many people lived into old age. The fifth chapter gives percentages of persons in dependent and able bodied groups in both plague and nonplague periods. The sixth chapter traces the spread of tuberculosis over the later Roman Empire and early Middle Ages through the heavy mortality of the years of age 15–40. The seventh and eighth chapters prove that, contrary to Biraben, the first plague (A.D. 542–ca. 750) devastated nearly all of Europe. These are important subjects.

Razi writes that, "Although only scientifically excavated skeletons from large cemeteries provide reliable demographic data [Russell] also employs data from small cemeteries as well as from unscientifically excavated ones." There are several safeguards from mediocrity in modern anthropological reports. Only 46 of the 562 reports were written before 1940 (p. 43). The heavy cost of excavation and publication required the support of learned societies and museums and caused these organizations to be particularly careful about their selection of scholars in their expeditions. Lesser skill normally resulted in lack of information rather than of distortion. That is, if the anthropologists could not separate children's skeletons by sex, they listed them as undetermined and little damage was done. Even in older work the statements about direction of graves was so simple that they would be used.

Razi has thus held up modern physical anthropologists to very unjust criticism. The size of cemetery is less important than the skill of the scholar: after all a bone is a bone is a bone. Let me illustrate the implication of Razi's thesis about little value of small cemeteries. The late S. Harrison Thomson thought from reading written sources that Bishop Robert Grosseteste was a small man. However, Grosseteste's skeleton was later identified as that of a man nearly six feet tall. Are we to throw out this evidence because it came from a tiny cemetery?

Razi apparently believes in the doctrine of Malthus (1766–1834) that population was controlled by subsistence and disease rather than by man's choice. Medieval numbers were reduced by wet nursing, late marriage, and heavy mortality (especially of women) between twenty and forty. Nevertheless women still seem to have produced about 4.2 children apiece, while only about 3.5 (p. 147) were needed for replacement. The one certain way to prevent increase in population was to cut down a sixth of the females at birth. This would produce a sex ratio of about 115 (men to 100 women). As late as 1424–27 Florence had a sex ratio in the first year of life of about 115, while Tuscany outside of the city had about 110. The skeletal life table of the period before the first plague (A.D. 542) was about 138, that of A.D. 570–1000 about 111 and of A.D. 1000–1348 about 134. However, during the first plague the ratio dropped to about 101. That is, when women were needed because of plague mortality they were allowed to live.

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The intensity of the will to control population is shown by the choice of method of control: infanticide was regarded as a sin by the Church and a crime by the state. This will was not a manifestation of a vague theory, but a very real desire to avoid future hunger within the family as it faced the problem of maintaining a female and her possible progeny over the next generation. One should not be surprised at human control of population. It has been a general rule until modern times.

Touching on peripheral subjects is always a problem. My decision was to limit even demographic topics to population control rather to discuss size of villages again. Many years ago I tried to estimate village population from court rolls and gave it up with regret. The rolls give much valuable and interesting social and economic information but are distressingly uncertain about residence of persons and numbers of children, even in Halesowen, studied by Razi himself.

JOSIAH C. RUSSELL
St. Augustine, Florida

PROFESSOR ZVI RAZI REPLIES:

Professor Russell has criticized my review of his recent book *Control of Later Ancient and Medieval Population*. He finds fault with my criticism of the demographic data obtained from small cemeteries and with my rejection of his analysis of population control during the period under study.

The skeletal data from small cemeteries unlike that from large ones, often do not provide a fair sample of the population. This can be seen clearly from the fact that while the numbers of male and female skeletons found in large cemeteries is almost equal, the number of male skeletons dug from small cemeteries far exceeds that of females.

In his letter Russell writes that, "Razi apparently believes in the doctrine of Malthus that population was controlled by subsistence and disease rather than by man's choice." Unfortunately here as well as in his book Russell grossly misrepresents Malthus's demographic theory. Malthus clearly argued that the population can be controlled not only by positive checks which increase mortality, like famines, wars and infectious diseases but also by preventive checks which lowered fertility such as a rise in the marriage age of women and in the numbers of women who never marry. Therefore, Russell's claim that the late ancient and medieval population was voluntarily controlled is not, as he thinks, incompatible with the Malthusian model. The reason I rejected his analysis of population control was not theoretical but empirical. The late ancient and medieval population might have been voluntarily controlled but much better evidence than that which was presented by Russell is needed to show that this was the case.

ZVI RAZI
University of Tel-Aviv

TO THE EDITOR:

I have to object to the incompetent review of my book, *No Longer An Island*, by Keith A. Hamilton (*AHR*, 91 [1986]: 404). He shows no understanding of a major new element in British life, the impact of air power on the British people and their governments, when he objects to my conclusion that "the Battle of Britain began when the Wright brothers flew." I pointed out in the book that when Winston Churchill sought to prepare his countrymen to withstand the shock of a German air attack in 1940, "the Battle of Britain," he told them that the fate of Christian civilization itself would turn on the outcome of the battle. I said if this impending attack was of such supreme significance it might be asked—properly—"When did the Battle of Britain really begin?" My conclusion, in this context, was: "A not unreasonable answer is that the Battle of Britain began when the Wright brothers flew." For ages past the British had been concerned with what Arthur Balfour called "this eternal and most important question of our safety against invasion." Their defense against invasion was the Royal Navy. As Sir Walter Raleigh, the official historian put it, "The British navy is a great trust, responsible not so much for the progress of the nation as for its very existence." When men began to fly, however, some few in Britain realized that Britain now lay open to an invasion from the air; and traditional reliance on the navy and sea power was no longer so valid as it had been in what was looked on as the dawn of a new age, the air age. Lord Northcliffe's genius enabled him to see this at once and it was he who said "England is no longer an island," the title of my book. The theme of the impact of air power on the British people may be traced in detail from these earliest days of successful mechanical flight to the Battle of Britain of 1940 when the mortal threat to the country was in the air and not on the sea. Indeed, the first government to attempt to purchase the Wright invention, the airplane, was the British Government. Given this context, it is ignorant of Hamilton to write about the invention of the coracle or the Viking longship in connection with the Norman Conquest.

Hamilton calls my book "inconclusive" because he wants to know the consequences of R. B. Haldane's

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refusal to purchase the Wright airplane. He asks: "Was Britain any less well prepared in 1914 for war in the air than her Continental neighbors?" The subtitle of my book is "Britain and the Wright Brothers, 1902–1909." It ends in 1909 because the dealings between the British Government and the Wrights come to a definite term in that year. I am preparing a sequel that treats the years 1909–1914. Major and complicated aeronautical developments took place in that period but it cannot follow that *No Longer An Island* is "inconclusive" because it does not deal with them.

Hamilton also complains that "documents are cited at length when summaries would suffice." These were documents never published before and they show the reactions of contemporaries to something entirely new in human history. They deserve to be reproduced at length as a contribution to our knowledge of a tremendous novelty in the life of mankind. He also objects that "footnote references are sparse" when there are scores of footnotes in the book. He further charges that "This book is written in a style that will probably appeal to the general reader." What have we here? Is he an academic who believes that history should not be written in a readable style? The impact of air power on the British people is an important but neglected theme of recent British life. It merits the most extensive attention, even if Keith A. Hamilton is incapable of grasping this fact. This is the subject of my book but he thinks, for some unimaginable reason, it is an "essay on weapons procurement."

ALFRED GOLLIN
University of California, Santa Barbara

PROFESSOR KEITH A. HAMILTON REPLIES:

Professor Gollin accuses me of showing "no understanding of . . . the impact of air power on the British people and their governments." I should like to point out that having spent most of my early years within a mile of the spot where Blériot landed, and in a house whose foundations had been shaken by German aerial bombardment, I fully appreciate the impact of air power on at least some British people. Moreover, I have done a little research of my own on this issue. Five years ago I contributed an article on the British diplomatic response to the development of Zeppelin's airships to *The International History Review*, and since then I have dealt with an aspect of the same subject in a biography of Sir Francis Bertie, which the Royal Historical Society will shortly be publishing. My purpose in criticizing Gollin's "remarkable conclusion" that "the Battle of Britain began when the Wright brothers flew" was not to deny the very considerable significance of the advent of aviation for the security of the British isles. I wished merely to demonstrate the banality of the platitude.

It is, of course, obvious that the battle of Britain could not have taken place without the invention of the airplane. It is equally evident that there were those in Edwardian England who recognized that their country might soon become vulnerable to aerial invasion and attack. The army general staff was particularly anxious to deny facilities "to those Powers who look to the command of the air to redress our superiority at sea." Nevertheless, the British government's reaction to the new challenge to Britain's insularity was on the whole a cautious one. In 1910 the Admiralty was, for instance, still primarily concerned with the possibility of the Germans utilizing their dirigibles for naval reconnaissance. And after the Great War had revealed the versatility and military potential of the airplane, there were aerial enthusiasts, who, like Sir Frederick Sykes (the Chief of the Air Staff), saw powered flight as a means both of forging tighter links with Britain's overseas dominions and of protecting its imperial frontiers. Air power may have created new perils for the defenders of Britain, the island kingdom, but it also presented new and exciting prospects for the proponents of Britain, the world empire. The air battles which were fought out above Kent in the summer of 1940 were an unhappy, but hardly inevitable, consequence of the experiments of the Wright brothers at Kitty Hawk.

My main objection to *No Longer An Island* remains, however, the inconclusive manner in which Professor Gollin has handled the dealings of the British military authorities with the Wright brothers. He suggests that the War Office missed a great opportunity in failing to purchase a Wright machine, and he implies ineptitude on the part of Haldane and his staff. Yet their conduct can surely only be judged if the results of their decisions are taken fully into account. That is why I asked whether Britain was any less well prepared in 1914 for war in the air than its continental neighbors? That is also why I referred to Professor Gollin's consideration of this transatlantic bargaining as "an essay on weapons procurement." Neither the Wrights nor Haldane appear to have been especially interested in the promotion of civil aviation.

I agree with Professor Gollin that the theme of his monograph is an important one. I was fascinated by some of his revelations, and I paid him an honest compliment when I wrote that his book would "probably appeal to the general reader." My hope is that he will use his sequel to provide answers to the questions which I posed in my review, and that in

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the meanwhile he will discard some of the verbal ballast that has so weighed down his latest volume.

KEITH A. HAMILTON
University College of Wales

TO THE EDITOR:

Professor Kalvoda's review of my book, *The Soviet Union and the Czechoslovak Army* (*AHR* 90 [1985]: 1236) does not address its central thesis. The book argues that the influence and interference of the Soviet Union in Czechoslovakia are sources of continual tension in the Czechoslovak armed forces. It goes on to demonstrate that the strain reached crisis proportions during the Prague Spring of 1968, leading to a subsequent purge of the military after the Soviet invasion. Professor Kalvoda concentrates instead on the historical details of the first two chapters of the book. I would like to take this opportunity to respond to his critique.

Professor Kalvoda is right to point out that several dates, taken out of context, constitute historical errors. For instance, the Czechoslovak Legions were indeed formed in 1914, but they were not recognized as autonomous and allowed to fight on the side of the allies until 1918. The point in telling this story was that the Legions came into conflict with Bolshevik forces. The Bolsheviks, of course, were not on the World War I battlefield in 1914. Similarly, Ludvik Svoboda was, as the book states, in Poland in May 1938. He did not attempt to organize forces on Polish soil, however, until 1939, after the second partition of Czechoslovakia. I apologize for the imprecise language in reporting some of the details of Czechoslovak history in the introductory chapters. The argument stands, but greater care most certainly should have been taken.

I do not accept Professor Kalvoda's contention that sources have been used without care. Every student of contemporary affairs in the Soviet Union and Eastern Europe must work without access to archives. This is why every effort was made in this book to use official and unofficial Czech sources, Soviet sources, American government accounts from declassified documents, Radio Free Europe and Radio Liberty accounts. Were we unwilling to work with these sources, there would be no research on the modern Soviet Union or Eastern Europe. The sources have been used very carefully. In fact, probably all too often, sentences take note of uncertainty. ("General Dzur is said to have stated" or "Another report contends," p. 5.)

Moreover, Professor Kalvoda's quarrel with my use of the term "Czechoslovaks," stating that there are no Czechoslovaks, only Czechs and Slovaks, is unwarranted given the considerable attention devoted to nationality in the book (pp. 11, 35, 41, 127, and so forth).

Finally, I have not failed to take account of the historic resistance of the Czech and Slovak peoples. The book, in fact, dismisses Czechoslovak "passivity" as an explanation for the failure to resist in 1968.

CONDOLEEZZA RICE
Stanford University

PROFESSOR JOSEF KALVODA REPLIES:

Unfortunately, in her letter to the editor Professor Rice adds more historical errors to those made in the book. Due to space limitations of my review, I pointed out only a few of the many factual errors contained in her work and, for the same reason, I cannot be more specific in this rejoinder.

Not the "Czechoslovak Legions," but the *Czech Družina* was sworn in in Kiev on September 28, 1914. Shortly afterwards it was sent to the front to fight on the side of the Allies. Since many volunteers from among the Czech and Slovak deserters and prisoners of war joined it, the battalion-size unit became the St. Wenceslas Regiment; and when the second regiment of Sts. Cyril and Methodius was formed, the unit was transformed into a brigade on April 17, 1916. The brigade was cited for bravery by the supreme command of the Russian army after its victory in the battle of Zborov early in July 1917. It captured 62 enemy officers, 3,150 soldiers, 15 pieces of artillery and many machineguns, most of which were turned on the enemy. In the course of the battle a young officer, Radola Gajda, demonstrated his excellent military skills and leadership ability. However, he was not the "Legion's leader" and it is not a "fact," as Rice asserts in the book, that he was "one of the most famous collaborators during the German occupation" (p. 46). The fact is that he was among those who opposed president Edvard Beneš's capitulation policy during the Munich crisis. Had he not been framed by "The Castle," degraded and removed from the army, and had he been the Czechoslovak army's chief of staff in the fall of 1938, it is highly unlikely that the capitulation policy would have been carried out.

In May of 1938 Ludvík Svoboda was teaching Hungarian at a military academy in Czechoslovakia. Although Professor Rice makes a reference to his book *Z Buzuluku do Prahy* (note 52, p. 253) in order to document events taking place during World War I, the book does not even mention those events. However, the source is relevant to her letter. On page 26 of his book Svoboda writes that he came to Poland in June 1939; on page 30 he publishes a document corroborating the information. Had Professor Rice paid more attention to scholarly works

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and less to propandanda generated by those who are known for falsifying history, she would have learned to know that in the summer of 1939 the Czech and Slovak military units were organized in Poland by Army General Lev Prchala. The latter was one of the provincial commanders and generals who opposed Beneš's decision to capitulate in the fall of 1938. He tried to organize a coup against the president and the government, but the plot was betrayed.

It is not true that I concentrated only on the first two chapters of the book. For example, I pointed out that Josef Hodic, whose work is quoted extensively in the discussion of the Prague Spring in the fourth chapter, was a communist agent who returned to Czechoslovakia, and that the "Sějna affair," discussed in the fifth chapter, was based largely on communist propaganda sources, while the defector-general's highly informative and relevant to the thesis of the book writings were not even mentioned.

How can one take seriously opinions and/or interpretations of someone who does not have the facts straight? In scholarly works on the Soviet bloc countries contemporary sources can be used effectively if one knows the relevant historical facts, is familiar with political theory and practices of the Marxist-Leninists, and is able to separate facts from allegations, propaganda and outright falsehoods. Political analyses, interpretations and opinions have to be based on facts and not on misinformation. Indeed, as long as ignorance of history and philosophical illiterary are prerequisities for academic success, the Marxist-Leninists will celebrate new victories.

JOSEF KALVODA
Saint Joseph College

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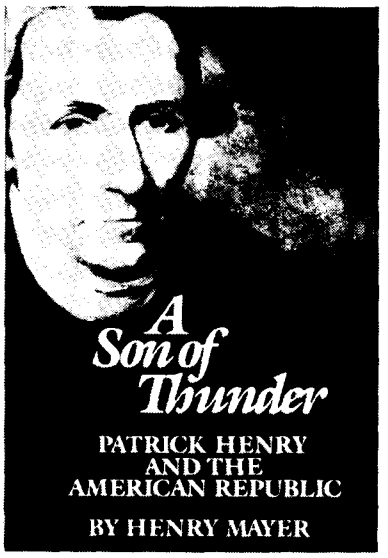
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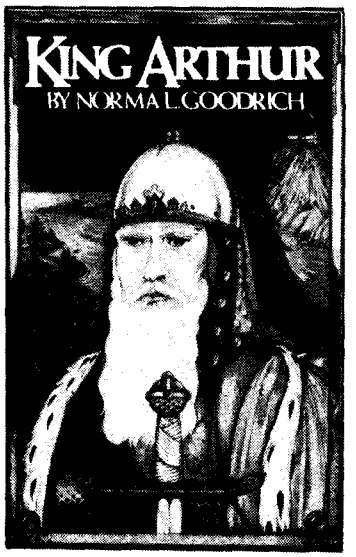
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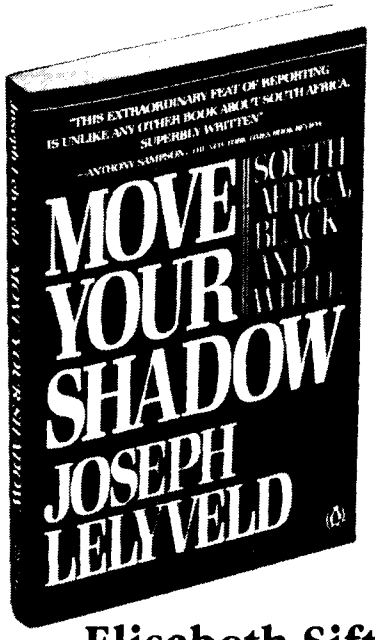
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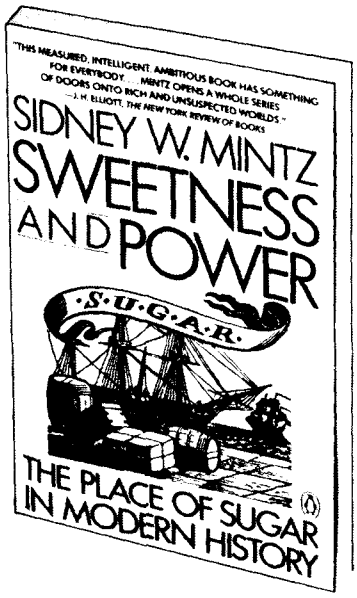
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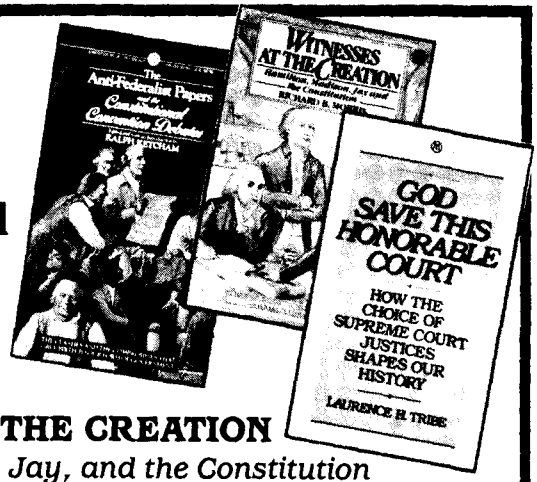


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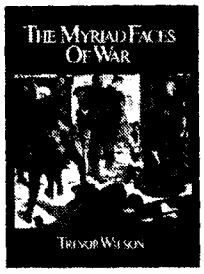
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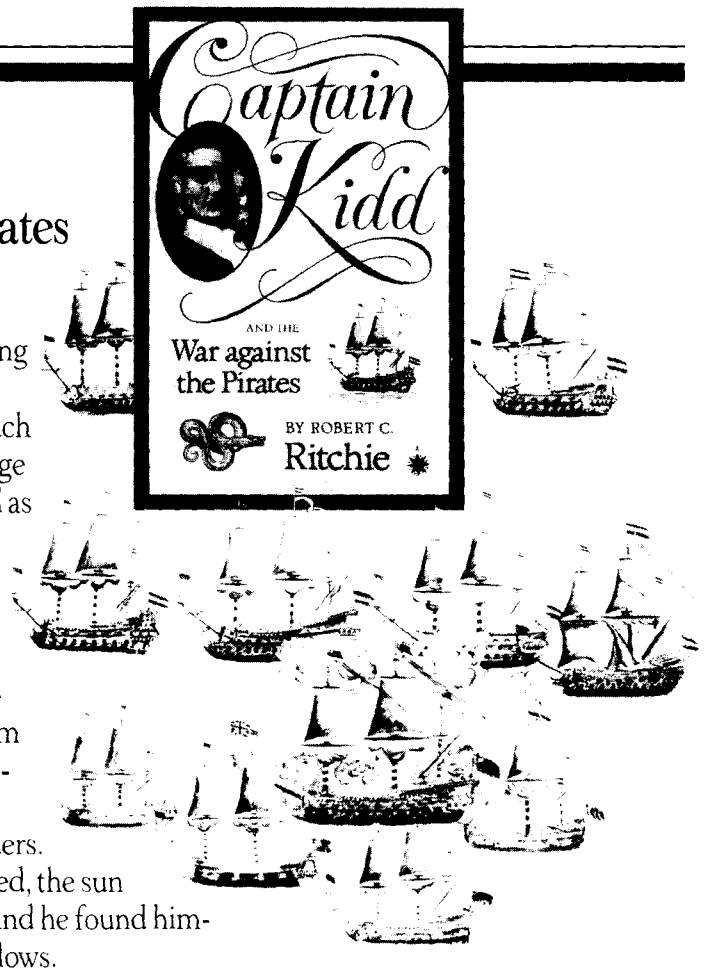
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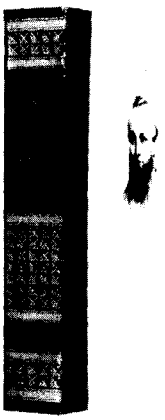
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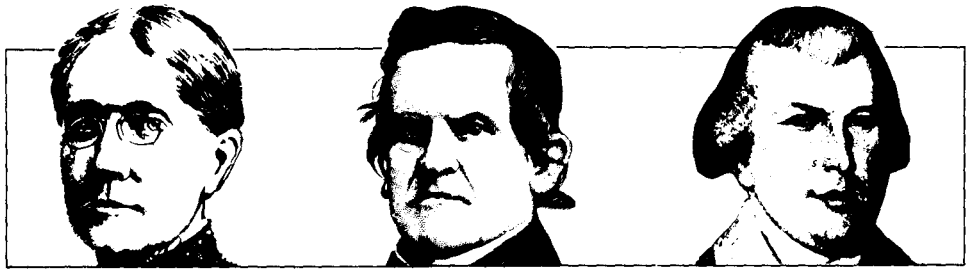
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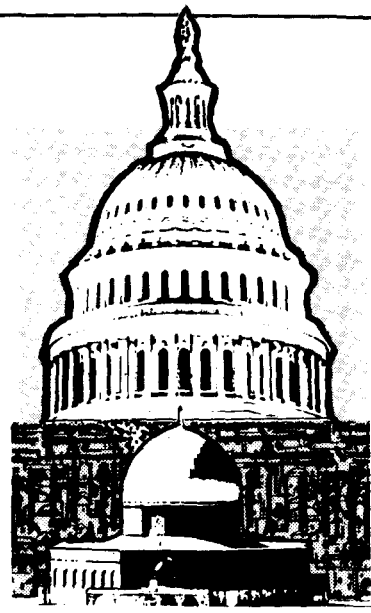
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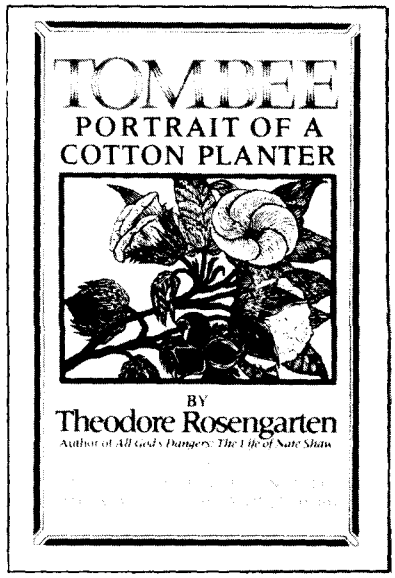
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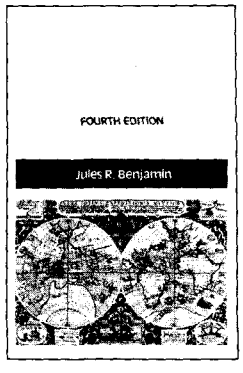
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